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The Nebulae around Novae

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From the earliest observed stage of the increase to maximum until some three magnitudes down the decline of light, every nova has a spectrum that may be described very generally as a stellar absorption spectrum with the addition of broad bright bands (really widened bright lines) of hydrogen and a few other elements.[1] But as the light fades still farther the continuous spectrum weakens, the absorption lines all disappear, and the composition of the remaining spectrum of broadened bright lines changes until it is practically identical with the spectrum of a planetary nebula. Every typical nova that has been observed spectrographically long enough after maximum light has developed the "nebular" spectrum. Of the repeating novae, T Pyxidis also had a well-developed nebular spectrum, but both RS Ophiuchi and T Coronae Borealis failed to produce a pure nebular type. The characteristic "forbidden" emissions of doubly ionized oxygen appeared but did not approach equality with the hydrogen lines.

Around the brightest (and therefore probably nearest) of the novae, small nebulae have been seen to develop during the years that follow the spectacular brightening. The record is most complete for Nova Aquilae 1918. Less than four months after light maximum, Barnard[2] detected a minute disc about the star, and by the summer of 1919, just a year after the outburst, the diameter was two seconds of arc, large enough to permit the partial separation of spectral features of different parts of the disc. The expansion continued uniformly until after 1941, when the nebula was so faint that it appeared unlikely it could be followed much longer. The nebula around Nova Persei was longer-lived, but it was first detected as late as 1917 when it had a diameter of 16 seconds of arc.[3] Since then the apparent rate of expansion has been uniform and it is evident that, just as with Nova Aquilae, the nebula was ejected from the star at or very close to the date of the maximum of light.

We still meet occasionally the suggestion that novae may develop into the well-known planetary nebulae. This is a fallacy that dies hard. At best it is supported only by a short-lived superficial resemblance of

[1] For a general account of spectral changes of a nova, see POPULAR ASTRONOMY, **52**, 109, 1944.

[2] *Ap. J.*, **49**, 199, 1919.

[3] Barnard, *A. J.*, **30**, 86, 1917. Pease, Ritchey, *Pub. A. S. P.*, **29**, 256, 1917. Ritchey, *Pub. A.S. P.*, **30**, 163, 1918.

the nova nebula to the planetary, and by a longer-enduring similarity of the central stars. But the planetary nebula, though probably not permanent, is certainly a rather stable object with a lifetime of many centuries, while the nova nebula at best lasts only a few decades. The Crab Nebula, a remnant of a *super-nova*, is the sole known exception to this statement. The spectra of planetary nebulae show that they are expanding only very slowly; in most of them the velocities are too small to produce clear separation of the lines emitted on the near and farther (approaching and receding) sides, and the most rapid known speed is 55 km/sec in the nebula NGC 2392.[4] The nebulae around novae, on the other hand, expand with speeds of hundreds of kilometers per second; Nova Aquilae had a velocity of 1700 km/sec, and the slowest known is Nova Herculis 1934, about 300 km/sec.

Retardation of the expansion of these fast-moving shells by the gravitational attraction of the star would be inappreciable at the great distances from the star already attained by the gases when the shell first becomes visible. The gas must continue outward with practically undiminished speed until it attains a low density approaching that of the interstellar medium. Long before this it would have become invisible. Then, in the course of many centuries, the atoms would be slowed down by collisions and would mingle with the interstellar gases.

A slowing down of the nova shell at an early stage by the resistance of the interstellar medium appears to be out of the question. There has been no measurable slowing in any of the nova nebulae that have been most completely observed. The interstellar medium is less dense than a planetary nebula by a factor of the order of ten thousand, hence there seems to be no possibility that the known planetaries have been slowed down from nova-like speeds. The calculated masses of planetary nebulae are of the order of one-tenth the solar mass, while the masses of several nova shells have been calculated to be smaller by a factor of about a thousand.[5] On every count the nova nebula fails to satisfy the requirements for the origin of a planetary nebula. There is no need to pursue the matter further; the evidence has recently been summarized by Minkowski.[6]

Between the early phases of the nova and the stage of the visible expanding nebula there is often a considerable hiatus. By inference we can bridge the gap. Near maximum the widths of the bright bands indicate the velocity of expansion of the gases about the star. Later, the velocity of expansion of the nebula is nearly the same. Or, lacking observations of the spectrum in this later stage, the *assumption* of equal velocity leads to a calculated distance and absolute magnitude that are consistent with results for other novae or from other methods. It is logical to conclude that the broad bright bands during the "stellar"

[4] *Lick Obs. Pub.*, **13**, 114, 1918.
[5] C. and S. Gaposchkin, *Harv. Circ.*, 445, 1942.
[6] *Ap. J.*, **107**, 106, 1948.

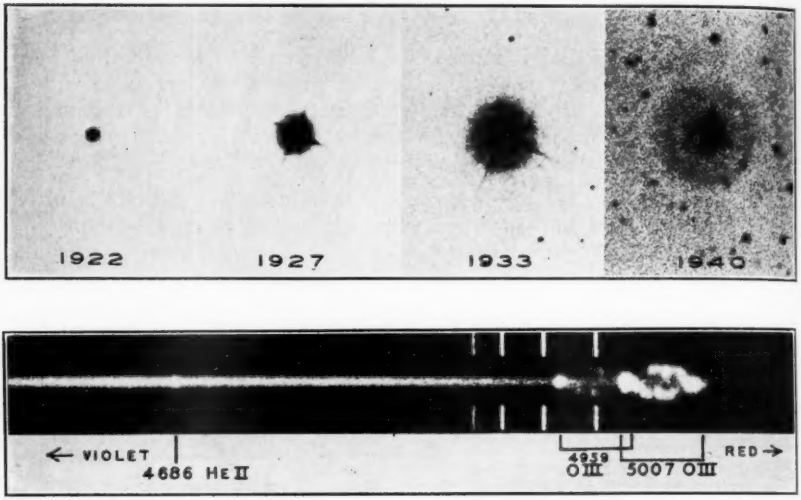
stage and the visible growing nebula are simply two different aspects of *one and the same* expanding shell of gas.

Nova Aquilae 1918 is perhaps the best example of a clear proof of the identity of the early expanding gases of the "stellar" stage with the later nebula. A detailed study was carried out by E. S. Pearson[7] at Cambridge, England, using spectrograms from Victoria. It sheds a flood of light on the meaning of the nova emission bands.

During a few days only after maximum light the broad bright bands due to hydrogen had developed very well-marked structural features. Within the bands there were several maxima and minima of intensity. The maxima might be interpreted as due to denser knots of gas travelling at various angles to the line of sight, the minima as simply less dense parts of the shell between the knots. Although many variations of relative strength occurred, the main maxima were identifiable on many spectrograms throughout the five months that the nova was observable following its outburst. When the nebular bright lines of doubly ionized oxygen appeared, they contained the same pattern of maxima and minima as the hydrogen lines, and when the latter became too faint to permit observation of their features, the further history of the structure was followed in the nebular lines.

After conjunction with the sun and about one year after maximum light, the same maxima and minima were still present in the bands, but localization of maxima in different parts of the nebula was beginning to be evident.[8] By 1920 the expansion had gone far enough to show the features very well separated. The disc had a uniform circular outline, but across it a brighter bar extended in a northeasterly direction.[9] The brightest knot in the Doppler ellipse (Figure 1) came from a part of the nebula just northeast of the star, and there can be little doubt that the bright bar was the location of origin of that knot, which had a velocity of approach of 1600 km/sec. From only a few days after maximum light, this bright maximum of intensity had been intermittently visible at the violet end of each emission band. Other maxima in the band structure also originated in local parts of the growing nebula, and each of the more prominent ones had been followed for a long time. There could hardly be a clearer demonstration of the continuity of a single shell of material. It must be evident that whatever later eruption of matter may have occurred, it was insufficient to modify in an important degree the structure of the main mass of gas that was erupted at maximum light. This justifies the assertion that, in Nova Aquilae at least, the nebula originated in a single "main burst" that was thrown from the star in an interval of at most a day or so, and possibly within only a few hours.

[7] *Annals, Solar Phys. Obs. Cambr.*, **4**, Part 3, 1936.
[8] Moore and Shane, *Lick Obs. Bull.*, **10**, 30, 1919.
[9] Pease, *Pub. A.S.P.*, **32**, 334, 1920.



Courtesy of *Sky and Telescope*

FIGURE 1

Top: The expanding nebula around Nova Aquilae 1918, taken at Mount Wilson. These are negatives. The central strong spot is the much-overexposed image of the star; the four rays are due to light diffraction by the secondary mirror supports.

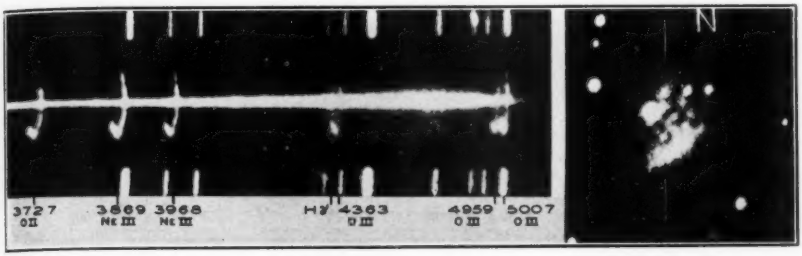
Below: The spectrum of Nova Aquilae and its expanding nebular shell, photographed with iron comparison lines at Mount Wilson in July, 1920, with a much greater scale than in the direct photographs above. The horizontal streak is the star's spectrum, continuous except for the bright knot 4686 of ionized helium. At the right are two Doppler ellipses of doubly ionized oxygen in the nebula.

Nova Persei 1901[10] had a marked pattern of maxima and minima in its emission bands; one especially strong maximum had a velocity of 650 km/sec approach. These features became conspicuous within a few days after maximum, nearly disappeared at times during succeeding weeks, and then reappeared in full force some six months after light maximum.[11] There is no doubt of the identity of structure throughout that interval. When last observed with sufficient dispersion (at Lick Observatory 18 months after maximum light) the same pattern was still present. A long interruption of the record then occurred. The growing nebula was detected in 1917, but not until 1933 was a good spectrogram taken.[12] The bright lines outlined a large part of a Doppler ellipse and the brightest section was seen to originate in the southern bright fan-shaped mass of nebulosity (Figure 2). Quite clearly this

[10] Campbell and Wright, *Lick Obs. Bull.*, **1**, 46, 1901; unpublished study of the spectrum by the author.

[11] For a suggested physical interpretation, see *Pub. A.S.P.*, **60**, 54, 1948.

[12] Humason, *Pub. A. S. P.*, **46**, 229, 1934.



Courtesy of *Sky and Telescope*

FIGURE 2

At left, the spectrum of Nova Persei (1901) and its nebular shell in 1934, photographed with helium comparison lines by Humason at Mount Wilson. The horizontal bright strip is the continuous spectrum of the star. Each bright "fishhook" is about half a Doppler ellipse in the light of one of the lines emitted by the nebula. At right, the nebula around Nova Persei in 1934, also by Humason. The fine line represents the position of the spectrograph slit when the spectrum was taken.

was the location of origin of the bright maximum in the early emission bands, for the velocity was about 650 km/sec approach. With the more continuous record of Nova Aquilae in mind, we feel justified in concluding that in Nova Persei also the nebula originated close to the date of light maximum.

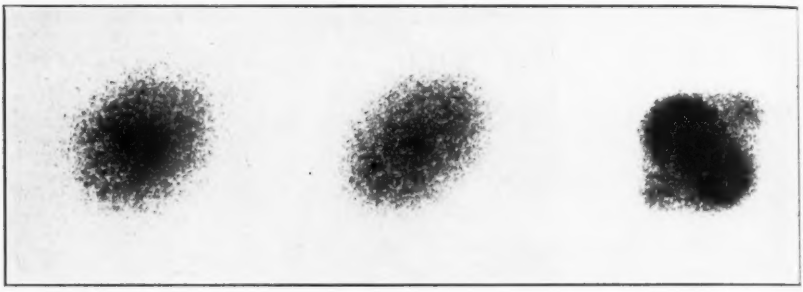
The history of the nebula about Nova Herculis confirms the general picture of a spheroidal shell with denser knots imbedded in it. In July, 1935, about seven months after the outburst, G. P. Kuiper[13] found that the nova, then in its nebular stage, appeared exactly like a close double star. An accompanying statement by Menzel pointed out that the duplicity doubtless referred to the nebula and not to the star; nevertheless, in some quarters the observation was taken to mean that actual fission of the star had occurred. Subsequent history demonstrated the complete falsity of this absurd conjecture. During the next several years[14] the two star-like components separated at a uniform rate, became larger and fainter, and were seen to be enveloped by a growing nebulous haze. By the latter part of 1941, nearly seven years after light maximum, all resemblance to stars had vanished, and the true star itself was visible midway between the two nebulous knots. It had been there all along, of course, but had been drowned out by the brighter light of the nebula.

The nebula was photographed by Baade[15] in 1940. The photographs failed to show the two knots of the apparent "double star" but showed instead a fairly uniform elliptical nebula with its longer axis in the same position angle as the line joining the components of the apparent

[13] *Harv. Annc. Card*, 339.
[14] *Kuiper, Ap. J.*, **86**, 102, 1937; *Pub. A. S. P.*, **53**, 330, 1941.
[15] *Pub. A. S. P.*, **52**, 386, 1940.

double. Photographs taken with a selective filter showed the star at the center of the ellipse. Its magnitude was 13.35, not yet faded to its pre-outburst magnitude of 14.6.

By August, 1942,[16] the nebula measured 4″ by 3″ and with difficulty it could be made out that it was a hollow shell or apparent "ring nebula," though the star tended to fill up the dimmer center. The most remarkable feature was the different appearance in different colors (Figure 3). When photographed in the light of the green nebular lines



Courtesy of *Sky and Telescope*

FIGURE 3

Nova Herculis and its nebular shell, photographed by Baade at Mount Wilson in 1942. Left: The star and nebula in blue light. Center: In the light of the "nebular" green lines of doubly ionized oxygen, isolated by a filter, which suppresses the star. Right: The nebula in the light of ionized nitrogen, showing the radical difference of distribution of the radiating atoms of this element.

it was almost a uniform ellipse. But when photographed in the light of the red lines of [NII] there was a much brighter bar along the *minor* axis of the ellipse and two fainter condensations out near the ends of the major axis. On closer inspection the green nebular [OIII] radiation was found to be weaker along the minor axis, just where the [NII] emission was strong. It is not at all probable that this represents segregation of the elements in different parts of the nebula. The difference of appearance is more likely due to conditions of density or excitation of the gas.

Swings and Struve[17] in 1940 took spectra of the two ends of the nebula separately in an attempt to determine the character of Kuiper's knots. There was little difference between the two spectra, and both showed velocities of approach and recession as well as zero velocity, corresponding to motion across the line of sight. Previously there had been speculation as to whether each of the knots gave *only one* of the strong maxima that had appeared at the edges of the emission bands during the later part of the "stellar" stage of the spectrum. The observation by Swings and Struve disposed of that hypothesis.

[16] Baade, *Pub. A. S. P.*, **54**, 244, 1942.
[17] *Ap. J.*, **92**, 295, 1940.

By 1942 the nebula was large enough to permit good separation of the spectra of different parts. Humason[18] found that the images of the nebular lines were complete Doppler ellipses and that the gases had a range of velocities from about 220 to 400 km/sec, both in approach and in recession, relative to the mean velocity of —32 km/sec. Spectra of the nebula taken with the slit crossing it in different directions showed stronger condensations at the locations of the two bright knots seen by Kuiper. The apparent contradiction of the visual and photographic observations doubtless arises from a combination of circumstances. Atmospheric disturbances cause the telescopic image to jump and waver, even in "good seeing." The visual observer can follow the tremors of the image and see finely resolved detail, but the photograph records the wavering image in all its positions, with resulting loss of sharpness. Then too, the human eye in some circumstances is remarkably sensitive to small contrasts of brightness. Undoubtedly the two nuclei in their early stages were much more compact than in 1940 and much brighter relative to the surrounding haze of the complete spheroidal nebula. By the time the nebula had grown large enough to photograph clearly, the definiteness of the two knots was at an end.

The record of Nova Pictoris is less satisfactory. Early in 1928, less than three years after maximum, Dawson[19] reported the nova apparently surrounded by a nebula one second in diameter, and soon after that, Finsen[20] announced that the star appeared double. Jessup,[21] however, observed simply a hazy disc without nuclei. Later in 1928 and until early in 1931, van den Bos and Finsen[22] measured the nova as an apparent triple star, and occasionally a fourth component was suspected. The true star appeared central in a nebulous haze, in which were imbedded two bright condensations roughly at position angles 70° and 230°. These moved outward with uniform speed. When first seen in 1928 they were almost as bright as the central star itself and each was 0".4 from it. By 1931 they were each 1".0 from the star and 2.5 and 3.5 magnitudes fainter. It was not possible to follow them longer.

It is most unfortunate that there was then no large reflector in the southern hemisphere with which to photograph the later stages of the nebula. The record was interrupted for 13 years. In the meantime the 60-inch reflector was installed at the Harvard Observatory station at Bloemfontein. In 1944 came the laconic announcement[23] that several exposures showed the image of Nova Pictoris distinctly elongated in position angle about 70° but with no diffuse nebulosity. The direction of elongation is the same as that of the line joining the two nebulous knots, and one wonders if the image photographed may have been, not

[18] *Pub. A. S. P.*, **55**, 74, 1943.
[19] *Harv. Annc. Card* 55.
[20] *Harv. Annc. Card* 64.
[21] *Harv. Annc. Card* 68.
[22] *Cape Obs. Annals*, **10**, Part 9, 155, 1931.
[23] *Harv. Annc. Card* 679.

the star, but the nebula itself, which had retained a fairly definite outline. At the rate of separation of the knots, they should have been about 8 seconds apart in 1944 (each 4″ from the star). As far as it goes, the record of Nova Pictoris is consistent with that of Nova Aquilae, Nova Persei, and Nova Herculis.

With these four examples before us, we need have little hesitation about interpreting the isolated observations of small nebulae about three more novae. Thus, Nova T Aurigae 1891 had a nebula 12″ in diameter when Baade[24] photographed it in red light in 1942. Nova Lacertae 1936 and Nova Cygni 1920 were photographed by Baade[25] in 1943 and found to have small nebulae with diameters of 3″.5 and 4″.3, respectively. It is inferred that these shells originated at the times of the outbursts and that they have grown to their observed dimensions with practically *uniform* speed.

These growing nebulae make possible the only reasonably accurate calculations of the distances of *individual* novae. The rate of expansion in seconds of arc per year can be measured on the direct photographs. If we know what *linear* speed this represents, we can at once calculate the distance of the nova. And at least a close approximation to the speed is given by the Doppler shift of lines of the near and far sides of the nebula. Spectrograms show that the distribution of velocities is that of a roughly spherical shell expanding with similar velocity in all directions from the star. If the velocities are exactly equal in all directions the spectral lines will be perfect ellipses and we can calculate the distance of the nova very accurately. If the speeds are unequal the ellipses will be distorted and there will be a corresponding systematic error in the calculated distance. The relation is the same as that which connects the transverse motion of a star with its proper motion and distance (or parallax):

$$\text{Transverse speed (km/sec)} = 4.74 \times \frac{\text{proper motion (sec. per year)}}{\text{parallax (seconds)}},$$

which can be solved for the distance in this form:

$$\text{Distance (parsecs)} = 0.211 \times \frac{\text{velocity (km/sec)}}{\text{rate of expansion (sec. per year)}}.$$

Once we know the distance, we can calculate the absolute magnitude at maximum and at minimum. The necessary data are available for only seven novae discussed in preceding paragraphs. The resulting distances and absolute magnitudes are given in Table 1.

The longest known survival of a nova nebula is that of the supernova of 1054 A.D. which is still visible as the Crab Nebula in Taurus. The spectrum has the elliptical lines that we associate with nova shells, and

[24] *Pub. A. S. P.*, **55**, 261, 1943.
[25] *Pub. A. S. P.*, **56**, 218, 1944.

TABLE 1

DISTANCES AND ABSOLUTE MAGNITUDES OF NOVAE

Nova		Distance		Absolute Magnitude	
		Parsecs	Light years	Maximum	Minimum
Aquilae	1918	430	1400	—9.3	+2.6
Aurigae	1891	800	2600	—5.3	+5.7
Cygni	1920	1470	4800	—8.9	+4.6
Herculis	1934	230	750	—5.5	+7.5:
Lacertae	1936	1350	4400	—8.6	+4.6
Persei	1901	480	1560	—8.4	+4: var
Pictoris	1925	500	1630	—7.3	+4.2

the velocity[26] of 1300 km/sec combined with the observed rate of expansion measured on direct photographs[27] gives a distance of about 1250 parsecs and absolute magnitude —16.5, probably the most reliable individual determination of the luminosity of a supernova.

The Crab Nebula is unique in other ways. Baade[28] has photographed it in selected regions of wavelength. Ordinary photographs which combine a long range of blue-violet spectrum show diffuse nebulosity with numerous superimposed filaments. Red light near Hα shows *only the filaments*, and infrared shows only the diffuse nebulosity. Spectra taken by Minkowski[29] record only a continuous spectrum from the diffuse nebulosity, while the filaments give bright lines, particularly the strong pair due to [NII] near Hα. The filaments form a fragmentary ellipsoidal shell surrounding an inner amorphous mass, and the spectrum lines of the filaments are fragmentary Doppler ellipses whose approaching and receding (nearer and farther) halves are about equal in brightness, indicating that there is no appreciable extinction of their light in passing through the inner amorphous nebulosity.

The only other known supernova remnant is a tiny faint patch near the site of Kepler's nova of 1604.[30] It is a mere fragment with a radial velocity of —200 km/sec.[31] Presumably the rest of the shell is too faint to be recorded.

The probable central star of the Crab Nebula has been identified, but the stellar remnant of Kepler's nova has not been found. In the case of Tycho's nova of 1572, neither star nor nebula has been detected, in spite of a determined search.[32]

Around just one nova a nebula of a totally different kind has been observed. Six months after the maximum of Nova Persei 1901, an apparently expanding nebula was photographed by Max Wolf and later by other observers.[33] The rate of growth, two or three seconds of arc per day, implied a velocity of the order of that of light, and Kap-

[26] N. U. Mayall, *Pub. A. S. P.*, **49**, 104, 1937.
[27] Duncan, *Ap. J.*, **89**, 482, 1939.
[28] *Ap. J.*, **96**, 188, 1942.
[29] *Ap. J.*, **96**, 199, 1942.
[30] Baade, *Ap. J.*, **97**, 119, 1943.
[31] Minkowski, *Ap. J.*, **97**, 128, 1943.
[32] Humason, *Ap. J.*, **88**, 230, 1938; Baade, *Ap. J.*, **102**, 309, 1945.
[33] Ritchey, *Ap. J.*, **14**, 167 and 293, 1901; **15**, 129, 1902.

teyn[34] suggested that the effect was due to the illumination of a pre-existing nebula by the spherical wave-front of light that left the nova at maximum. If the motion were assumed to be perpendicular to the line of sight, a distance of about 100 parsecs followed at once. This interpretation received wide acceptance for many years, until Humason's observations of the spectrum and the direct image of the true expanding gaseous nebula showed that the distance must be about five times as great. Then the two phenomena, the great "light-echo" on the one hand and the real expanding shell on the other, stood in apparent contradiction. It was even hinted that the gaseous nebula might represent simply an excited *region* in a much more rapidly expanding cloud, and that the large distance of Nova Persei was an illusion! But it had long been clear that the "light-echo" alone could give no unique solution. The condition for simultaneous arrival at the earth of light simultaneously emitted by the nova is that the reflecting particles lie on the surface of an ellipsoid with the star at one focus and the earth at the other. By applying this principle and adopting Humason's determination of the distance, Coudere[35] showed that the nebula that gave the "light-echo" consisted of approximately plane sheets lying between the earth and the star at about 46 light-years from the latter and inclined about 45 degrees to the line of sight. This interpretation, however artificial it may appear, is demanded by the facts.

What if the nebula had been beyond the nova? Not only would the growth of the echo have been much slower, but the nebula would have appeared fainter. Light is more strongly scattered forward than backward; as illustration we have only to look at the sky near the sun on a slightly hazy day and compare its brightness with that of a region far from the sun. Even better, we may observe the intense fire of a cloud just above the setting sun, as contrasted with the pale pink of one overhead or to the east.

Visual observations of nebular shells may be attended with a good deal of difficulty if a refractor is used. The various bright-line radiations come to focus at different distances from the objective in the best achromatic refractors, and the larger the telescope the more pronounced this effect is likely to be. With photographic refractors there may be a serious effect of this type. Such instruments are corrected for the center of the range of wavelengths that are transmitted by glass and to which ordinary blue-sensitive plates react. Under these circumstances the strong green nebular lines at one end of the photographic spectrum and the strong ultraviolet line of [NeIII] may be appreciably out of focus when the greater part of the continuous spectrum of the star is in focus. A spurious disc will then result. Extreme caution is necessary in the interpretation of observed discs. Only a reflector can safely be said to give a reliable photograph of a nova shell.

[34] *Astr. Nach.*, **157**, 201, 1901.
[35] *Ann. d'Aph.*, **2**, 271, 1939.

In their early stages the nebular shells outshine their central stars for the same reason that a planetary nebula outshines the star at its center. The temperature of the star is of the order of 50,000° K, and most of its radiation is far out in the ultraviolet. The hydrogen of the nebula shines by the same fluorescent process as in the planetaries: atoms are ionized by absorption of far ultraviolet radiation, but on recombination with electrons they emit the several series of lines of the hydrogen atom. While free, the electrons collide with doubly ionized oxygen atoms and give up part of their energy in exciting the oxygen to metastable states. The oxygen eventually loses this excitation energy by emitting "forbidden lines," the well-known green "nebulium" radiations. Similar excitation by collision accounts for the red [NII] lines.

Although continuous ejection of gas from the nova continues for weeks or months after maximum, it is now fairly clear that the greater percentage of the matter erupted by a nova leaves the star during a very short interval of time near the date of light maximum. The main knots or condensations in this "main burst" or "principal shell" retain their identity for years while they travel outward at uniform speed. There is no evidence of considerable changes of velocity and there is no reason to call upon rotation to explain any of the features of the spectrum. In short, our geometric picture of the nova shell appears fairly correct in its broader features. The physics of nova shells is simply a special case of the physics of stellar atmospheres and planetary nebulae, and the changes of the spectrum appear to be in general accord with what would be expected of a mass of gas decreasing in density and moving farther from the hot central star. A great many details surely remain to be worked out. For example, we do not yet have a satisfactory explanation of the different distribution of [OIII] and [NII] emission.

Considerably greater mystery surrounds the processes that occur close to the central star. Well-known spectral features that can be assigned to this deeper region[36] indicate that continuous ejection operates during the later "stellar" and "4640" stages, and probably considerably longer, but the record of events is either interrupted in time or obliterated by the radiations of the surrounding nebular shell. And below these imperfectly observed levels lies the unobservable region whence atoms issue with high velocities.

Whatever the original cause and whatever the physical processes that operate, there is no reliable evidence of a great permanent change in the star itself, for the luminosity, years after light maximum, is about the same as before the outburst, and in a few instances the star has been able to repeat the performance after a few decades.

THE OBSERVATORY, UNIVERSITY OF MICHIGAN, MAY, 1948.

[36] McLaughlin, *Pub. Mich. Obs.*, **8**, 192, 1943; *Pub. A. S. P.*, **59**, 244, 1947.

Solar Bookkeeping

By JAMES C. BARTLETT, JR.

"The average life of a sunspot may be taken as two to three months . . ."; this from Young in both the 1881 and 1895 editions of his great work on the sun. Newcomb, writing in 1878, was more cautious. Said he: "Their duration is . . . extremely variable, ranging from a few days to periods of several months." Webb inclined to favor the long-period hypothesis, and mentions the famous "spot" of 1779 which was visible for six months.

At the risk of committing lese majesty, the writer can only say that if such spots as Young mentioned are *average* they have entirely escaped his notice during the course of a study of the sun for a number of years. Would it be surprising to hear that from two to three *days* is much nearer the truth?

If this is so, what then? Have the spots in recent years undergone a change of habit? Very probably not. It is much more probable that the word "spot," when used by the older writers, was often used loosely and interchangeably with "group." Groups may, indeed, persist for several rotations of the sun, but the spots of which they are composed are much more ephemeral. Several years ago the writer determined to investigate the life expectancy of the spots as distinguished from the groups. He has since become a bookkeeper to the sun who has indeed proven a hard master. The work is, in fact, so exasperating, that no one but a hopeless sunspot addict would embrace it.

There is only one way in which an accurate account of sunspots may be kept; and that is by making daily drawings of all the groups visible, drawing in each spot—to the tiniest pore—in each group, all in proper relation; then each spot—also to the tiniest pore—must be given a serial number which is written up in a ledger in true bookkeeping fashion, its duplicate appearing on the drawing for the day. If now we compare our drawings day after day, taking due note of the way the serial numbers are running, we may at a glance see just which spots have disappeared from any group and which are new spots to which new numbers must be applied. This too, of course, gives us a more or less accurate notion of the duration of the individual spots. Photography, of course, can substantially lighten this burden; though unless the photographs be on a scale not commonly attainable for amateur observers so many smaller spots will be lost that the visual method becomes superior.

Let us begin with the ledger. As the writer employs it, it is ruled off into five columns as follows: Column 1 bears the date of the observation; column 2 is devoted to the spot serial numbers, one number to each horizontal line of the column; column 3 is for all spots appearing in the northern hemisphere while column 4 is for southern hemi-

sphere spots; lastly, column five is reserved for the age of the spots. An observation is entered by first noting the date; then follow the serial numbers of all spots visible on that date. Let us say that spot No. 11518 is in the northern hemisphere. If so, a cross is placed in column 3 opposite that spot's serial number. Spot No. 11528 we find is in the southern hemisphere, so a cross is placed in column 4 opposite the serial number of that spot and so on.

Several days later we notice that spot No. 11518 has disappeared. We now turn to our ledger and look it up, finding that it was first visible on say the 15th of the month. It is now the 18th and we last saw it on the 17th. It has, therefore, disappeared sometime between the 17th and the 18th. Accordingly we enter in column 5 a life of 2 days which is the age of that spot.

At the end of the year if we take the first serial number entered for that year and subtract it from the last serial number entered, *adding one*, we have the true number of individual spots observed during that year. If now we total the crosses in both columns separately, we have also the distribution of individual spots by hemisphere of all visible—or rather observed—for that year. Next we add the individual ages together and divide the sum by the numbers of spots, the quotient being the mean or average age of the spots. It may be disconcerting to hear that of 3,639 individual spots observed at Baltimore in 1947—a very active year—the average age was only 1.1 days.

What does this prove? Simply that it is *the agitated area of the photosphere* in which they are manifest, and not the spots themselves, which is the important element. This area is the seat of the group, and as such it may keep a group in being for several weeks while the group's spots are dissolving and forming constantly from day to day. Of course, it must be remembered that any average or mean figure sometimes conceals as much as it reveals; and while the mean spot-life for over 3,000 spots was only 1.1 days in 1947, many of these spots had *actual* lives of from 5 to 13 days. Two individual spots, belonging to the great S.H. group which appeared in the first week of March, 1947, had lives of 61 and 63 days, respectively; but they were by far the longest-lived of all spots observed that year.

But the vast majority of individual spots had actual lives which came within one or two days of the mean age of all spots, which is to say they had actual lives of from two to three days. Hordes of them had actual lives of only one day, and very many of them had actual lives of less than a day. Those spots which persisted for periods ranging from 5 to 13 days were in most instances the leaders and followers of groups, which, as is well known, commonly outlive the spots of the bridge or of the penumbral field in an irregular group. The highest daily mortality rate, for instance, is commonly found among the small spots in a large penumbral field group; the lowest rate is for those in the bridge of a simple bipolar group . . . always excepting leader and follower.

In a large penumbral field, it is common for scores of very small spots to have actual lives of less than a day.

It might be thought that no special difficulty should attend such determinations—but alas! Consider the following:

A simple bipolar group comes around the following limb on a given date and is seen to consist of 25 individual umbrae, arranged as leader, bridge, and follower. Three days later the group is seen to consist of 35 umbrae. Going now to our ledger, in which the original 25 have been duly numbered and entered, we are able to determine which are the new spots (having regard also to our drawing). We therefore enter for *that* day, *i.e.*, the third day, the new spots. What could be simpler? Ten new spots have appeared and we have tagged them. But *have* they? When we first saw the group it was at an extreme angle and we have no means of knowing how many spots may have been lost to us through the effects of foreshortening. Perhaps the apparently new ten were, at least in part, present from the beginning.

From the very beginning, therefore, an element of uncertainty enters into our laborious bookkeeping, which is not an auspicious way for anything to begin. But let us persevere. Now that the group is fairly entered upon the disc the effects of foreshortening may be neglected. For a few days we are able accurately to number and enter all new spots which do appear, and to enter the ages of those which disappear. All is going well—until one fine day the barometer is observed to be dropping. That night a thin film of cirrostratus overspreads the sky and our spirits decline with the mercury in the barometer. A period of rain and overcast follows and we do not see the sun again for five days. When at last we can examine the group again, consulting our drawing of last date of observation, we find that *all* of the spots—save follower and leader—previously entered have vanished. What was their duration? We have no means of knowning, and so must sorrowfully enter a string of question marks opposite their serial numbers. That group, so to speak, has been a washout.

A new perplexity now appears. When at length we see the group again, we note not only the disappearance of many spots but the appearance of new ones in their places. But at what time during the five day interval of rain and cloud did they appear? Some undoubtedly appeared on one day and some on another—but that is all we can venture to guess. Subsequent observations of these new spots must therefore also contain a considerable element of uncertainty. We can do nothing more but write them up as appearing—or as being first seen—on the first clear day after the catastrophe of rain and cloud. If now they disappear two days later, our record will show a life of two days; but this is really only a life of 2 days after first being seen. Their actual life may have been nearer seven days.

Fortune favors us and we are able to observe the passage of the group around the preceding limb. Since leader and follower have sur-

vived transit across the entire disc our spirits rise a little. At least we are able to fix an accurate age for *those* particular spots; but a little reflection shows that we are scarcely in better case than before. Disturbing questions present themselves. How long had those spots persisted *before* they came around the following limb? We don't know. Worse. How long will they persist after having passed around the preceding limb? Again we don't know. Our record will show an actual life of some 13 days, but the true life may approach 39 days. If such bookkeeping were done in a bank, the chief cashier would be in bad repute 24 hours after the first audit.

Next we are entertained by a vast penumbral field group, which comes around the following limb with every sign of making a lengthy stay. Resigning ourselves to inaccuracies inherent in the effects of foreshortening, we rejoice when at length the group is entered fairly enough upon the disc to disclose itself more or less broadside. A laborious drawing is made of loops and trains of tiny umbrae to say nothing of the multitude of dots which appear in the field itself. Each is given its serial number and duly written up in the ledger. The weather this time remains clear and we settle down to really accurate accounting.

Next day we observe vast changes, after the manner of such groups, and eagerly compare our previous drawing and our ledger. We find that some 95 individual spots were written up on the day preceding, most of them extremely minute. Therefore we have our work cut out for us. First we examine the trains and loops. Here determinations are fairly easy; but when we turn to the penumbral field we are in different case.

First we note that the total number of dots in the penumbral field is about the same as yesterday; but the pattern of arrangement is altogether different. Are these then all *new* spots? Or have the tremendous currents in the penumbral field merely shifted existing spots about? We have no means of knowing. The only way in which this could be determined would be to keep the sun under almost constant observation from rising to setting, and this is impossible . . . at least for the average observer. However, the penumbral field spots are now all in different positions and some appear larger than any seen yesterday while others seem smaller. We therefore enter them as new spots and note a life of less than a day for their predecessors observed yesterday —but a vexing doubt remains and again our bookkeeping is seen to be uncertain.

Matters continue thus for the entire transit of the group across the disc and we resign ourselves to approximations for the umbrae in the penumbral field, excepting only those large enough to have such distinct individuality that no doubt can be entertained. Meanwhile we have been watching some minute dots which accompany the leader and which

are free of the penumbral field. When first seen there were three of these dots arranged in an arc.

Two days after first observation the three dots have vanished. We enter a life of two days for them and turn to other things. Five days after this, we notice the appearance of three more dots, arranged in the same arc, and occupying more or less the same position relative to the leader. We are about to enter them in our ledger as new spots when a horrible doubt occurs. Are they really new? Or are they the same spots which may have been veiled by overlying vapors, or perhaps by the masking action of flocculi not otherwise sensible to direct view?

They *might* be—and again they might *not* be. A little sadly, we take a chance and write them up as new spots. Presently the vast group passes around the preceding limb. Within two weeks it is back again, and we compare our last drawing with the one just made of the appearance on return. As to the penumbral field, which is still going strong, we simply give up and write down all the umbrae contained as new spots; but we are able to recognize the leader and also the follower. We are, therefore, able to give a relatively accurate account of those particular spots. Still, we don't know how long they had persisted on the invisible hemisphere before first appearance at the following limb; so our satisfaction is much diminished.

But when last seen, the group had also contained several fairly large umbrae exclusive of the hordes of dots in the penumbral field and the many small umbrae arranged in trains and loops. Perhaps we can recognize some of them. Again we compare drawings carefully. Well, here is a spot which occupies *approximately* the position of one seen previously. But it is larger—or perhaps smaller—and the general configuration is altogether different. Is it then the same spot merely altered in appearance? Or is it a totally new spot which has replaced one which occupied nearly the same position? Who can say? Accordingly as we judge from appearance and probability it is entered as a new spot or merely continued as a former spot; and we are at least thankful that we do not have to submit reports to the State Auditing Office.

Then comes a day when everything seems favorable. A small group of scattered spots appears in the central zone on the day following a given observation. Here, at last, is a group the beginning of which we are at least enabled to fix within definite limits of time. We settle down to watch, keeping a wary eye on the barometer. But the weather holds up and all is well.

Two days after first appearance the entire group has vanished. We enter a life of two days for the spots it contained (assuming all to have remained to the end) and congratulate ourselves that at last we have made as accurate a determination as possible. Four days later, an almost identical group appears in the same latitude but now near the preceding limb. Since an interval of four days has elapsed since the disappearance of the original spots, these must certainly be written up

as new spots; but grave questions immediately arise. Suppose that they are the same spots, perhaps masked for four days by flocculi. If so, they are *not* new spots at all. Yet they are new in the sense of making a new appearance. How shall they be entered? Again, we remember the theories which hold that spots disappear and later reappear in the same positions. If we are dealing with such a case, how again are we to consider these spots? Are they really "new"? Or are they merely new *appearances* of the same spots? We give it up and write down new serial numbers, hoping that posterity will be charitable.

Enough has been said to indicate just a few of the difficulties attending solar bookkeeping as applied to sunspots. Obviously, large errors must inevitably enter into estimates of spot longevity. It is clear too that this element of uncertainty must be taken into account when we establish the mean life periods of all the spots at the end of each year. It might be wondered, therefore, if any value at all can be attached to such means.

The answer—rather surprisingly—is a qualified "Yes." Fortunately, over the course of a year, sufficiently long periods of sustained observation make it possible to fix the ages of very many individual spots within definite limits. These alone, if averaged, show a very low mean value for the life expectancy of an average sunspot. Summarizing, we may say with some confidence that an age of 5 to 6 days is quite advanced for moderately-sized spots with perhaps 10 days allotted to larger spots; while periods ranging from less than a day to a day are all that can be claimed for small umbrae, pores, and dots. Only the very largest spots persist individually through two rotations of the sun, and since they are relatively infrequent and their numbers correspondingly small the *mean* age of sunspots taken over a year is not sensibly increased by them.

It must be understood that there are many exceptions to these rules. The writer, for instance, has often observed spots of modest dimensions which outlasted by many days spots much larger. He has also occasionally noted minute umbrae which became veritable Methuselahs among their kind, some lasting for 5 days or more. But they are in all cases the exceptions which prove the rule, and the overwhelming majority of spots have the kind of life spans suggested above.

Brief inquiry may now be made as to the relation of spot longevity to the type or size of spot. That such a relation exists is readily apparent by consulting ledger and drawing. Leaders and followers commonly—though not invariably—have the longest lives of any spots in the group, and it is these spots which most often persist at least for one transit across the disc and which, for all we know to the contrary, may actually represent the completion of one rotation of the sun upon its axis.

Next in order come the larger spots of the train or bridge, having actual lives of from 2 to 3 days and sometimes as much as 5; rarely

as much as 10. The minute umbrae, which occur in clusters, sprays, or trains accompanying the larger spots, rarely exist for more than a day and often for less than a day. They, in turn, are followed by the minute dots which sometimes appear exactly on the periphery of the penumbra of a large sunspot. Most ephemeral of all appear to be the tiny umbrae enmeshed in a large penumbral field. They appear, on the whole, to have lives averaging less than a day. As such spots occur in the greatest number it is clear that they will act to bring down the mean value of longevity for all the spots totalled at the end of a year.

Paradoxically, among smaller spots at least, the most active have the shortest lives. But the same thing is occasionally noted among the very largest umbrae. In such a case the umbrae soon splits into a number of fragments which sometimes undergo further division, and so the original spot disappears as such, its substance as it were being distributed among many offspring. On the other hand spots which develop slowly and more or less evenly decline in the same manner, and may confidently be looked to for relatively long life spans. In somewhat the same manner, the spots comprising a small bipolar group have much longer individual lives than those of a very active and very large penumbral field group. Perhaps longest-lived of all are small single spots which develop quite leisurely, persist more or less unchanged for many days, and then gradually become smaller and smaller until, like the Cheshire cat, nothing is left but the grin—and in this case not even that!

If a comparison is made between the mean ages of spots during a minimum period with those of a maximum period, it is found that the mean average age of the spots is somewhat higher for the minimum period—though not markedly so. It might be supposed that during a minimum period the spots have longer actual lives.

To some extent this is the case, the reason being that during a minimum period the spots are most often of a quiescent type and slow in development. We have seen that spots of this type commonly have longer individual lives than those of a more violent and active type. On the whole, however, the mean age is elevated during a minimum period by a substantial lowering of the number of spots visible. Since this number is employed as divisor to the total of indvidual spot ages at the end of the year, it follows that the quotient will be proportionately larger and the mean age of the spots will seem to be higher. On the other hand, during a maximum period, practically every observing day will be a day with spots visible. The divisor, therefore, will be much larger. Moreover, the dividend will be smaller by virtue of the vast number of minute umbrae which commonly appear in the great penumbral field groups characteristic of such a period, and which have lives averaging less than a day. The quotient, consequently, will be smaller and the mean age of spots will seem to be lower. However, there is very little difference between the mean ages of the two periods, so that when all is considered your average sunspot is an

ephemeral thing indeed. As with nations among men, so with groups among sunspots. It is the *group* which persists, while the individual spots play their brief rôles and vanish forever.

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Triple Planetary Conjunctions of Venus and Jupiter

By R. B. WEITZEL

The usual direction of planetary motion relatively to the fixed stars is towards the east. Venus moves eastward until the first stationary point is reached, from twenty to twenty-three days before inferior conjunction, then the apparent motion of the planet changes from east to west and this reversal continues until the second stationary point is reached, from nineteen to twenty-two days after inferior conjunction, whereupon Venus resumes its regular eastward motion. It may be noticed that the retrogradation of Venus happens only near inferior conjunction. On the other hand, the retrograde motion of Jupiter begins sixty days before solar opposition and ends sixty days after opposition. Consequently, the motion of Jupiter near Venus and solar conjunction—long after opposition—is not retrograde but direct, with a daily variation in geocentric longitude of approximately twelve minutes or one-fifth of a degree.

When Venus and Jupiter are at solar conjunction within sixteen days of each other, there are three conjunctions of the planets in a period of four months.

A table of solar conjunctions of Venus and Jupiter within sixteen days of each other, from 1791 to 1994, is presented. The columns give in order, year, month, day, hour in Greenwich Civil Time, Julian day number for Venus, place of Sun, and difference in time between the solar conjunctions of Venus and Jupiter.

Solar Conjunctions of Venus and Jupiter

Venus	Conjunction	d	h	G.C.T.	☉	Diff. d	Diff. h	Jupiter	Conjunction	d	h	☉
1791	X	19,	19	2375501.79	206°.20	—14	12	1791	X	5,	7	191°.98
1795	I	2,	5	2376672.21	281.52	+ 3	11	1795	I	5,	16	285.40
1815	X	13,	9	2384260.38	199.15	+ 1	11	1815	X	14,	20	200.58
1836	VII	26,	0	2391852.00	123.04	— 5	22	1836	VII	20,	2	117.32
1860	VII	19,	1	2400611.04	116.52	+10	10	1860	VII	29,	11	126.47
1881	V	3,	10	2408204.42	43.08	—10	20	1881	IV	22,	14	32.56
1905	IV	27,	10	2416963.42	35.46	+ 6	20	1905	V	4,	6	43.09
1926	II	7,	15	2424554.62	318.10	—13	9	1926	I	25,	6	304.53
1950	I	31,	7	2433313.29	310.85	+ 3	13	1950	II	3,	20	314.43
1970	XI	10,	10	2440901.42	227.57	— 1	3	1970	XI	9,	7	226.47
1991	VIII	22,	19	2448491.79	149.17	— 5	0	1991	VIII	17,	19	144.35
1994	XI	3,	0	2449660.00	220.30	+14	20	1994	XI	17,	20	235.24

The foregoing table treats of solar conjunctions which condition

triple planetary conjunctions. The following table gives the dates of the initial, central, and third conjunctions together with their approximate geocentric longitudes. Asterisks are used before half the central conjunctions to indicate invisibility of Jupiter because of nearness to Sun.

TRIPLE PLANETARY CONJUNCTIONS

First Conjunction				Central Conjunction				Third Conjunction			
		d	h			d	h			d	h
1791	VIII	6,	13 179?7	1791	XI	5,	21 198?6	1791	XI	24,	6 202?3
1794	XI	9,	9 272.8	*1794	XII	27,	7 283.7	1795	III	5,	2 298.3
1815	VIII	17,	12 188.6	*1815	X	12,	4 200.0	1815	XII	11,	11 212.4
1836	V	20,	5 104.5	*1836	VII	31,	6 119.8	1836	IX	17,	16 129.9
1860	VI	1,	16 114.3	1860	VII	9,	6 122.0	1860	IX	28,	5 139.1
1881	II	20,	14 18.7	1881	V	13,	7 37.5	1881	VI	20,	17 46.0
1905	III	5,	18 29.6	1905	IV	21,	12 40.1	1905	VII	5,	11 57.1
1925	XI	26,	20 291.3	1926	II	22,	5 311.1	1926	III	19,	2 316.5
1949	XII	7,	20 301.3	*1950	I	28,	1 312.8	1950	IV	4,	9 327.9
1970	IX	12,	0 214.4	*1970	XI	11,	11 226.9	1971	I	5,	6 238.4
1991	VI	18,	11 132.0	*1991	VIII	27,	2 146.5	1991	X	15,	18 156.6
1994	IX	29,	23 224.9	1994	X	15,	2 228.0	1995	I	14,	21 247.2

The first planetary conjunction occurs on direct motion of Venus when the planets are evening stars; the second planetary conjunction occurs during retrograde motion of Venus when the planets are evening stars, provided that the planetary conjunction takes place before inferior conjunction of Venus, otherwise the planets are morning stars; the third planetary conjunction occurs on redirect motion of Venus when the planets are morning stars. Jupiter is invisible in half the cases of central conjunction; likewise, Venus is invisible except in the cases where its greater brightness serves to offset the twilight coefficient of extinction.

Near solar conjunction, Venus has a stellar magnitude of —3.4, and Jupiter of —1.0, with the result that Venus is 2.4 magnitudes brighter than Jupiter. This difference in magnitude, converted into light intensity, shows that Venus is nine times as bright as Jupiter; but the latter is brighter than any fixed star save Sirius, and is fifteen times as bright as Mars when that planet is near solar conjunction.

Some approximate intervals separating the tabulated conjunctions, expressed in years with two decimal places, may be mentioned: 3.27, 20.75, 24.02, 44.77, 155.08, and 199.85. Other intervals beyond the tabular range are: 310.16, 485.99, and 510.01 years. A combination of the last two intervals produces an excellent long-term cycle of 996 years or 363782 days.

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Telescope Tripods

By H. B. RUMRILL

There is as much variation in the tripod mountings of small refractors as in the telescopes themselves. With portability dominating, the result has been a large output of tripods lacking more or less in the rigidity requisite for finest performance.

As a typical instance of structural frailty in a tripod I recall an attempt by a friend to secure a photograph of a moving steamboat on the Delaware River. The instantaneous exposure was successful, but he had first tried to get a time picture of the scenery as background. The very slight effort made in carefully changing the shutter mechanism had imperceptibly disturbed the position of the camera, causing a shadowy overlapping of the scenic setting—enough to blur the desired effect and spoil the plate.

The once popular "pillar-and-claw" mounting, with its clinging altitude motion, that cannot work positively without the addition of worm gearing—and usually minus means for balancing the tube—is of limited usefulness astronomically, however serviceable it may be as a "lookout telescope." It is mostly when made with an overhanging fork, or as a simplified equatorial, that it may be applied to the study of astronomy, as otherwise objects in the zenith are not accessible.

Good design and consistent proportioning of a tripod stand are helpful toward assuring reasonable absence of shake and "back-lash" in the handling of the telescope, especially with magnifications not exceeding 30 or 40 to the inch of aperture. The criticism of essaying higher powers really lies partly in any stand that is deficient in this respect. Doubtless, *absolute* rigidity is not completely attainable with any mounting having the telescope attached at or near its center, although most closely approached in an iron column resting on solid masonry.

Many of the books dealing with observational astronomy treat the matter of tremor as a necessary evil; easily understood by examining the illustrations in detail, most of them patently such as could not possibly appeal as those of instruments capable of yielding the utmost satisfaction in use. Some seem to be so slender and spindly as to invite this bane of the observer, and when added to trouble of similar character arising from wind and atmosphere it is no wonder that the pleasure that should belong to astronomical observing is much curtailed. Even if unpropitious weather had little influence as a deterrent from direct study of the stars, the ardent student of astronomy might be excused when preferring arm-chair observation because of its comfort, not caring to put up with the bother otherwise inescapable.

The first thing that strikes anyone having engineering instinct is that the older mountings were frequently too weak to properly serve their intended purpose and often requiring some sort of strengthening to

provide resistance to tremor, avoidable by simply making the stands of substantial dimensions in the first place. As comparison, with the evolution of the microscope came the gradual adoption of mountings with low center of gravity plus sufficient stability in the movable parts to show firm images with high power lenses.

Attempts to gain quietude in telescope performance by means of "steady rods" (as with the English "garden stands") and other devices of make-shift character are an acknowledgment of inherent defects, and only partially successful because of disregard of another element — that of weight, which is of course not compatible with ready portability. But why expect the latter with telescopes more than three inches in aperture? Certainly, object glasses of greater light-gathering capacity deserve at least semi-permanent mountings that will guarantee performance approximating that of a good microscope, experience with which has given the writer a standard by which to gauge the possibilities of telescopic observation at its best.

Along this line it is well known that the original mounting of the Washington equatorial had to be replaced by the massive one characteristic of modern instruments. It is hard to imagine that the still older Dorpat telescope, with its wooden framework, could possibly have been equal in performance to the same aperture on a Warner and Swasey columnar mounting, a single glance at which would suffice to prove its superiority.

On the other hand, the writer believes that the semi-permanent mounting of telescopes of four or five inches aperture can be achieved with well-made tripods, necessarily too heavy for ordinary handling, and with some advantages peculiar to that type, including broader base spread. Circumstances having decided the adoption of a tripod stand for my four-inch telescope, constructed at first of portable weight *when not moved too often*, it did not take long to discover that reenforcement at a definite distance below the head would be necessary to ensure minimum tremor even with medium powers. The solution appeared to lie in a design involving centralized weight, partly accomplished by means of steel bolts passing through mortises in the legs, and screwing into a cast iron ring at the base of a thick brass tube projecting downward eighteen inches from its fastening underneath the support of the equatorial mounting. These bolts have lock-nuts pressing against the central ring and the insides and outsides of the tripod legs; the ring itself resting on a flanged bushing that terminates the vertical tube.

One feature of this mounting is a "dumpy" tripod (mentioned in Bell's "The Telescope"), the leg dimensions being 4½ feet length, of solid mahogany 2¼ x 3½" at top, tapering near the bottom to 2¼ x 2½". Another important feature is the insertion of screw bolts in the leg bottoms, by which great nicety of adjustment is possible, enabling good leveling, with incidental correction of the polar axis and of the azimuth setting. The latter is placed initially by the fixation of a heavy brass

sub-head, to which the equatorial head proper is fastened, by a flanged screw bolt passing through the tripod top vertically, with a washer and nut beneath. In thus setting the azimuth position this screw bolt is first tightened when the polar axis is in the meridian, as determined by repeated testing of the motion of stars in the central field of a high power eyepiece.

The tripod rests upon a wooden triangle of accordant dimensions, with the pointed ends of the bolts fitting into conical borings in thick bronze base-plates, the angle the standard 60°. Heavy lock-nuts augment the upward pressure of the screw bolts—these and the entire outfit of bolts (including the lower end of the declination axis) adjustable by means of a key made of quarter-inch steel rod fitting at convenient places, and very easily applied.

In erecting the tripod in my observatory the legs were placed at an acute angle in order that the telescope might be readily directed to the zenith. Foot blocks fix the points where the legs rest, set accurately at the 120° angle. Under the ends of the triangle one-inch holes were drilled to a depth of a half-inch, in which steel balls are tightly pressed, also exactly set at the same angle. The utility of this arrangement was evident when the triangle was carefully laid on the concrete pier, in which while still slightly plastic the balls made indentations. As the weight of the telescope mounting is quite sufficient to hold it in position in this way there is no necessity for fastening the triangular base with bolts as with pillar mountings.

All this was of somewhat theoretical character, taking up one principle at a time as each factor contributed to making the motion of the telescope non-resilient. Inadequate weight, however, was still a fault, along with too high a center of gravity, to remedy which a cast iron triangle was made so as to rest on brackets bolted solidly to the inside of the tripod legs, and on this two cast iron discs—total weight of these parts 200 pounds. Through the superimposed weights a one-inch hole was bored for the insertion of a screw bolt, fastened top and bottom by washers and nuts. Between the weights are sheets of lead—a useful metal for absorbing vibration.

Still working empirically, I added a wooden upright cylinder wedged symmetrically over the iron weight system, in order to further stiffen the whole structure. Possibly not quite "orthodox" from an instrument maker's standpoint, the construction in its entirety, combined with precision workmanship, has solved the problem almost ideally, making it easy to operate the focussing mechanism and change eyepieces with scarcely noticeable tremor, as well as allowing the application of magnifying powers up to the point of atmospheric and light-quantity limitations.

Following my own plan of having the principal parts "over-size" (despite the advice of a friendly machinist who made them, but who was not familiar with telescope manipulation), although subordinating

graceful appearance to practical utility, has proven no mistake in the sequel. But the utilitarian has long since dispossessed mere "looks" in instrument design and construction, setting its own standards of beauty. In other words, the small telescope may to its advantage emulate the larger in the essentials that make for the excellence associated with increase of aperture.

When correctly balanced in both co-ordinates, clamping the telescope in position with a power of, say, 120, sometimes extended to 300 or more, the object stays "put" with the movement of the right ascension handle and is motionless except for the gradual change of position caused by the earth's rotation. In looking at nebulae and star clusters, for instance, with powers as low as 50 or 60, one would imagine the objects to be nearly at rest, or rather drifting slowly across the ocular field, such is the solidity of the mounting, which it must be confessed implies some form of observatory, not being in the portable class.

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The Partial Solar Eclipse of November 12, 1947

By VICTOR A. ESTREMADOYRO

Following is a report made to the Peruvian Astronomical Association. It was translated from the Spanish by Mr. Antonio H. Obaid, a member of the teaching faculty of Carleton College.

Although somewhat belated, we felt it desirable to have a record of the data here given concerning an eclipse of which very few observations were made. EDITOR.

Three eclipses have taken place during the year of 1947, two solar and one lunar. Only one of these eclipses has been visible under favorable conditions from this capital (Lima), namely, the partial solar eclipse of November 12. The solar eclipse of May 20 was visible from this capital as a partial eclipse very early in the morning and under very unfavorable conditions. I personally was able to see it for a brief moment in a clear spot among the clouds. The lunar eclipse was invisible for us. Hence the only eclipse of importance to us this year is the one reported on herein, for whose observation I made preparation with eager anticipation.

The program of observations which I drew up included the following items:

1. The determination of the time of contacts and therefore the duration of the eclipse, as well as the determination of the time of occultation and reappearance of the different groups of solar spots. For this purpose I began to synchronize my watch with the time broad-

casts from the Naval School of Peru several days before the phenomenon took place.

2. The photographic record of the different phases of the eclipse. In order to determine the proper exposure, I first took some solar photographs. I also prepared the dark room in order to obtain the best possible results.

3. Thermometric readings in order to determine the temperature variations during the eclipse and comparison with readings obtained previously. For that purpose I recorded thermometric readings for almost a month and drew the corresponding curves.

Having drawn up this program, I waited for the day of the eclipse, but unfortunately that day was completely cloudy in Chorrillos, as well as in Lima and surroundings, and so, in order to observe the eclipse, I had to set out in search of a place where the atmospheric conditions might be favorable. I decided to go to Chosica. Before setting out I synchronized my watch with the Naval School time and then I went to Lima, leaving there at 1:15 P.M.

At a short distance from Lima—about 6 to 8 kilometers—the sky was somewhat cleared up, and in the neighborhood of Vitarte the conditions of visiblity were very good. From the bus I noticed that it was cloudy towards Chosica, and, knowing that there the visible portion of the sky is very limited due to the narrowness of the valley, I decided to remain near Pariache, where I was able to make the observations with excellent conditions of visibility. During the entire time there was not a single cloud to disturb me in the least.

The approximate geographic coordinates of the station are latitude 11° 59′ S., longitude 76° 50′ W.

Unfortunately I could not carry out my whole program of observation as I would have done if I had been able to observe from Chorrillos. Therefore I had to be satisfied with the observation of the contacts and occultation and reappearance of the different groups of solar spots.

In making the observations I used my little prismatic lens, with a 4-cm. aperture, using the projection method. Before the announced time of the beginning of the eclipse, I drew up a chart on which the different groups of solar spots were shown. As I was operating out in the open, without a dark chamber, it is almost sure that many small spots went by unnoticed by me.

The groups of solar spots and the number of spots in each one of them are as follows: group *a*, 13 spots; group *b*, 5 spots; group *c*, 11 spots; group *d*, 1 spot; group *e*, 1 spot, and group *f*, 4 spots; a total of 6 groups with 35 spots. Of these 6 groups 3 are in the central zone of the sun, 4 in the north hemisphere and 2 in the south hemisphere.

The first contact of the eclipse occurred at $15^h\ 27^m\ 27^s$. Then I began to take the times at which the occultations of the different groups of solar spots took place, and I obtained the following results.

Beginning of occultation of group *a*: $15^h\ 54^m\ 8^s$. End of occultation of

group *a*: $15^h\ 59^m\ 49^s$. Beginning of occultation of group *b*: $16^h\ 06^m\ 40^s$. End of occultation of group *b*: $16^h\ 08^m\ 55^s$. Beginning of occultation of group *c*: $16^h\ 15^m\ 08^s$. Beginning of occultation of group *d*: $16^h\ 49^m\ 04^s$. Beginning of occultation of group *e*: $17^h\ 05^m\ 01^s$. The group of spots which I have designated as *f* was not covered by the moon, for it passed almost tangentially.

Since I already had the record of the different spot groups, their reappearance was not to catch me by surprise, but rather I could, with certain anticipation, expect their reappearance from behind the body of the moon. The times of reappearance of the different groups follow.

Reappearance of group *a*: $17^h\ 05^m\ 48^s$. Reappearance of group *b*: $17^h\ 20^m\ 21^s$. Reappearance of group *c*: $17^h\ 21^m\ 27^s$. Reappearance of group *e*: $17^h\ 33^m\ 33^s$.

The last contact, end of the eclipse, took place at $17^h\ 55^m\ 16^s$, a little to the north of the group which I have designated *d*.

From the data we conclude that the duration of the eclipse was $2^h\ 27^m\ 49^s$, the maximum phase reaching a magnitude of 0.71, taking the diameter of the sun as the unit. This phase took place at approximately $16^h\ 41^m$.

Back in Chorrillos I kept on synchronizing my watch with the time broadcasts from the Naval School at 20, 21, 22 o'clock, with which I determined the exact time and thus I arrived at the times mentioned above.

These data have been partially corroborated by the data obtained by our associate, Mr. Ulrich Petersen, who went to Ricardo Palma to perform his observation. Unfortunately he could not follow the development of the entire phenomenon because sunset occurred relatively early, due to the fact that the hills of that region form a very narrow valley.

These then are the results which I have obtained from the observation of the partial solar eclipse of Nov. 12, 1947.

CHORRILLOS, PERU, NOVEMBER 15, 1947.

Radio and the Stars*

By SIR EDWARD APPLETON, F.R.S.

I have just been reading again the original papers of K. G. Jansky, the American radio worker, who first discovered that radio waves come out of the Milky Way. I remember that I read them when they were first published, at intervals of twelve months or so, in the early 'thirties. The story of his discovery was striking enough when read with a year or two in between the chapters. But it is still more striking when read at one sitting.

*Published also in the British magazine, *The Listener*.

As so often with brilliant scientific work, it was a case of the scientist recognizing and following the unexpected. Jansky was not looking for radio waves from the stars at all. He was looking for something much more pedestrian. He was working on the subject of atmospherics, those unwanted noises with which we are all so familiar. He had erected an elaborate aerial system which told him the direction these atmospherics were coming from. He was certainly thinking about thunderstorms and lightning flashes as possible sources, and one of the first things he found was that he could account for most of his crashes and noises in terms of either local or distant thunderstorms.

But there was still something more. When other noises were at a minimum there remained a weak hissing noise in the telephones when his aerial system was adjusted to receive from a particular direction. That was the first thing he did not expect. Then he found that the source of this noise seemed to move steadily throughout the day. And the next morning it appeared to have moved round so as to be back in its old position again—which is, of course, what the sun would do. So Jansky's first surmise was that the mysterious noise originated in the sun. Then he made more accurate measurements of the time and found that the source of radio noise was not exactly in the same place again after twenty-four hours, but after twenty-four hours less four minutes. In other words the important interval of time was not a solar day but a sidereal day—a very different thing. This indicated that the radio noise was not coming from the sun at all but from a fixed direction in space. It was not very long before he had satisfied himself that this particular direction was the direction of the Milky Way.

But when you look at the Milky Way on a clear night you get the impression that there is a lot of it. It is not a single source like the sun. It is a flat pancake with the stars concentrated in the central plane. So we want to know whether different parts of the Milky Way emit radio waves more strongly than other parts. We can see for ourselves that there is a big difference in visual brightness between the different parts. That is because when we look at the Milky Way we can examine one bit of it without seeing the others. Unfortunately, when we try to do the same kind of thing by radio and make the aerial of our radio receiver act like a radio telescope, we cannot pick out the radio detail in the same way. That is the source of the whole trouble. It is true that if we use a very short wavelength and a very large aerial collector we can restrict the spread of reception to a small angle. But it is found that galactic noise gets weaker on the short wavelengths. So one cannot go too far in that direction. No, we have just got to recognize that our radio aerial not only looks straight forward, but also looks out of its eye-corners to some extent as well.

So, on the experimental side, effort in the study of galactic radio noise has been directed towards mapping out the strength of the noise

coming from different directions, using apparatus which necessarily smudges the results, since it cannot take in the fine detail. You have to correct for the smudging, mathematically, afterwards. In this radio mapping of the heavens there was very little done before the war except by Jansky himself. Indeed it is true to say that his discovery was neglected by other people. But during the war, when radar receivers were operated on short wavelengths of the order of five meters, many people re-discovered the phenomenon of galactic noise for themselves. Interest has now become intense and the subject is really fashionable.

I think we can claim that the most reliable radio mapping of the heavens has been done by three British workers. Hey, Phillips, and Parsons, of the Ministry of Supply, who did their experiments in Richmond Park, using a modified radar receiver working on five meters. They have produced a detailed map indicating the strength of the galactic noise coming from all directions in the sky. Their map, of course, confirms Jansky's main discovery that the noise comes generally from the Milky Way, but they have also worked out in greater detail the characteristics of the two major sources, the main one in Sagittarius and the minor one in Cygnus. Another British worker, Mr. Moxon, of the Admiralty Signal Establishment, has examined how galactic noise varies in strength as the wavelength of the receiver is varied. This relation is important from a theoretical standpoint.

Let us turn to possible explanations of this remarkable and unexpected phenomenon. The first person to speculate on this subject was Jansky himself. He said that the one possible explanation of the origin of galactic noise is that the radio waves come from the stars themselves, and that the reason the marked radio emission comes from the Milky Way is that there is a very great density of stars there. The only way we can check up on the essential feature of such a theory is by examining our own particular star, the sun itself. And, at the time when Jansky raised the matter, that is in 1935, no radiation from the sun had been detected in the radio spectrum. So that did not look very promising.

Fortunately, however, since 1935 it has been discovered that very strong radio emissions can be received on the earth's surface from the sun. But they tend to have their maximum intensity when there are sunspots on the sun's disc. The solar radio noise, in fact, is found to originate in the sunspot region itself. That is proved by the fact that although strong radio noise is received when a sunspot is visible it stops when the spots go out of sight due to the sun's rotation.

So we can say that, if we regard our sun as a sample of the stars in the Milky Way, it will be the source of radio waves with a cycle of intensity of just over eleven years, corresponding to the well-known sunspot cycle. But, of course, if we suppose that the galactic radio noise from the Milky Way is really the integrated effect of the radio

emissions from the individual stars we need not worry about sunspot cycles, because with so many stars, things would average out. There would always be some stars in a spotty state. So that presents no difficulty.

No, the real difficulty that has been encountered with this theory is to account for the remarkable strength of the galactic noise. We know roughly how strong the radio emissions are from the sun. Very well, let us assume that all the stars in the Milky Way radiate as strongly. Are there enough of them to conspire to produce the effect which is noticed? Some American workers—Greenstein, Henyey, and Keenan—have examined the matter and concluded that there are not. They conclude that the radiation from the stars themselves would be a million million times too small. But of course we cannot rule out the theory entirely since it may be that the stars in the Milky Way emit radio waves far more strongly than the sun itself.

If we do not accept the view that the galactic radio noise comes from the stars themselves we are bound to consider that it comes from the spaces between the stars. At one time it was thought that this interstellar space was empty, but evidence has gradually accumulated showing that it contains flying atoms and electrons and also dust. The subject was first discussed in detail by Sir Arthur Eddington in his now famous Bakerian Lecture delivered in 1926 before the Royal Society. We know that there is dust, because the light from distant stars which has to traverse a great depth of space is slightly reddish, due to the fact that the dust particles scatter blue light more effectively than they do the red. You get the same effect at sunset when you observe sunlight through clouds and through the dust haze that is always present near the ground. It is possible to estimate the size of these dust particles in interstellar space: they are about a millionth of an inch across. But I do not think we need seriously consider dust particles as contributing to galactic radio noise. Any effect they might have can be shown to be far too small. But it is quite a different matter with the clouds of atoms and electrons in interstellar space.

Stonyhurst College Observatory

By P. J. TREANOR

The closing of Stonyhurst Observatory, Stonyhurst College, England, on December 31, 1947, brings to an end more than a century of scientific work. Founded in 1838, the Observatory has been staffed and maintained by the Society of Jesus. Its first work was purely meteorological, and this has been continued throughout its existence. The Royal Society Commission, which in 1867 replaced the Board of Trade in the organization of meteorological observations in Britain,

selected Stonyhurst as one of the seven principal stations, each of which it supplied with standardized equipment and continuously recording instruments. For many years these stations formed the mainstay of British meteorology.

Stonyhurst Observatory was, however, early in its existence, to extend its activities to geophysics and astronomy. At the instance of Sir Edward Sabine, regular magnetic measurements were started by Perry in 1861, and continuous magnetic recordings a few years later. During the years 1868-70, Perry and Sidgreaves carried out an extensive magnetic survey of France and Belgium. In more recent years, seismology has also been included in the regular program.

Astronomical work had already engaged the attention of Stonyhurst when the Observatory gave refuge to Secchi during the Italian troubles of 1848. Perry's interest in solar physics defined the direction which this work was to take. In 1881 he commenced the series of daily solar drawings, which were included in the routine of the Observatory for sixty years. To these drawings Sidgreaves and Cortie added the spectroscopic study of sunspots. In their pioneer research on the B-D region, which supplemented the work of Lockyer and Smart, they were among the first to detect molecular bands, and to identify the numerous vanadium and titanium lines. In addition, Cortie found time to use the material provided by the simultaneous solar and magnetic observations for an independent analysis of the interrelation of solar and geophysical phenomena.

During the years 1870-1914, the Stonyhurst astronomers played leading parts in a number of scientific expeditions, including both the transits of Venus (1874 and 1882), and seven solar eclipses. Astronomers will remember in particular the Royal Astronomical Society's eclipse expedition to French Guiana in 1889, which Father Perry led to a successful conclusion only at the cost of his life.

The considerable program of solar and geophysical work and the relatively modest equipment of the Observatory made a continuous program of stellar work impracticable. The occasional researches, mainly on the spectra of novae, carried on within these limitations were not for that reason desultory or ephemeral, but showed at times an eye for experimental technique of which a larger institution would not have felt ashamed. Stonyhurst was one of the few observatories to secure photographic spectra of Nova Aurigae in 1892, the first nova to be so photographed. For this purpose Sidgreaves and Cortie used a slitless spectrograph with a collimated beam, instead of the objective prisms used at Harvard and at South Kensington. In this they anticipated Wadsworth's suggestion by a decade, and its common adoption by a still longer time. A similar ingenuity stamped the attempt to apply Orbinsky's length-of-spectrum method to the measurement, from objective-prism spectra, of the radial velocity of the expanding shell

of Nova Aquilae (1918). The astronomical work of Stonyhurst will always be associated with the name of Perry, but scarcely less credit is due to the originality of Sidgreaves, and the effective collaboration of O'Connor. While not so great an astronomer, Cortie contributed much to the spread of astronomical knowledge among a wider public.

The growing disparity between modern observational needs and the limited equipment of a private observatory has caused Stonyhurst in more recent times to return to the geophysical and meteorological work for which it was originally founded. This selfless task has been performed for many years by Father J. P. Rowland, the retiring director, whose advancing years and failing health have precipitated an inevitable though regretful decision. He will have the satisfaction of having completed a century of recording, and will terminate, temporarily at least, a line of priest-scientists whose solid achievement in their self-imposed task will be most readily admitted by their fellow physicists and astronomers.

The Planets in December, 1948

By RAYMOND H. WILSON, JR.

NOTE: The time employed is Central Standard Time unless otherwise indicated. The phenomena have been chosen and described for the North American continent, and especially for the United States. The basic data have been taken principally from the *American Ephemeris and Nautical Almanac*.

Sun. During most of this month the sun will be more than 23 degrees south of the equator, with the result that, at high noon, it is only 27 degrees above the horizon for an observer in 40 degrees north latitude. However, it will start north again on December 21 at 5 P.M., the Winter Solstice.

Moon. The phases of the moon will occur as follows:

First Quarter	December	8	8 A.M.
Full Moon		16	3 A.M.
Last Quarter		22	11 P.M.
New Moon		30	4 A.M.

Perigee will occur on December 20.

Evening and Morning Stars. Jupiter will disappear from the evening sky during the first part of the month. Morning stars will be Venus and Saturn, the latter rising about midnight.

Mercury. Since it comes to superior conjunction on December 12, Mercury will be practically unobservable this month.

Venus. Although still brilliant in the morning twilight, Venus will gradually become less favorably situated for observation.

Mars. Except for the first three days, when it might be picked up by proximity to Jupiter and the moon, Mars will be practically unobservable.

Jupiter. Even at the beginning of the month this bright planet sets at 5:30 P.M., so it will be difficult to observe. On December 2 it will be only a degree

north of Mars and 6 degrees south of the thin crescent moon, thus forming part of a group which might be noticed.

Saturn. Saturn will be the only planet really easy to observe this month, as a conspicuous star in the constellation Leo. On December 21 at 3 A.M. it will be just 3 degrees south of the waning moon.

Uranus. Uranus will be moving slowly westward, about 4 degrees northeast of ζ Tauri.

Neptune. Neptune will be moving slowly eastward at a point almost half way from γ Virginis to Spica.

Department of Mathematics, Temple University, Philadelphia, Pa.,
October 8, 1948.

Asteroid Notes

By HUGH S. RICE

We offer herewith the positions of the brightest minor planets available at this time. Planet 63 is in northern Taurus, 130 in Eridanus, 192 in Auriga, and 2 traverses northern Caelum. The visual magnitudes are, respectively, about 10, 9, 8, and 7.

ASTEROID EPHEMERIDES
For 0^h U.T.

1948	α h m	δ ° ′
	63 AUSONIA	
Nov. 9	4 14.8	+30 18
17	4 6.1	+30 8
25	3 56.9	+29 48
Dec. 3	3 47.9	+29 20
11	3 39.8	+28 45

1948	α h m	δ ° ′
	130 ELEKTRA	
Nov. 9	3 53.8	−16 39
17	3 47.6	−17 5
25	3 41.3	−17 8
Dec. 3	3 35.4	−16 48
11	3 30.0	−16 7

1948	α h m	δ ° ′
	192 NAUSIKAA	
Nov. 9	4 58.9	+35 26
17	4 51.5	+35 43
25	4 42.2	+35 44
Dec. 3	4 32.3	+35 28
11	4 23.0	+34 58
19	4 15.2	+34 16

1948-49	α h m	δ ° ′
	2 PALLAS	
Nov. 9	5 2.9	−27 57
17	4 58.3	−29 55
25	4 52.3	−31 21
Dec. 3	4 45.5	−32 16
11	4 38.5	−32 38
19	4 31.8	−32 28
27	4 26.1	−31 47
Jan. 4	4 22.0	−30 34

Hayden Planetarium, American Museum of Natural History,
New York, N. Y., October 20, 1948.

Occultation Predictions for December, 1948

(Taken from the *Amerian Ephemeris*)

The quantities in the columns *a* and *b* are given for the purpose of making these predictions useful for any place within 200 miles of the point indicated. The procedure is as follows: Subtract the longitude of the point given from the longitude of the place in question; multiply the result in degrees, taking the

signs into account, by the quantity under *a* for the star to be observed; similarly, with the latitude, using *b*; apply the sum of the products, with its proper sign, to the Greenwich C.T., and obtain the predicted Greenwich Civil Time for the phenomenon at the place of observation. To obtain Eastern Standard Time it is necessary to subtract five hours; Central Standard Time, six hours, etc.

Date 1948	Star	Mag.	IMMERSION Greenwich C.T.	*a*	*b*	Angle E from N	EMERSION Greenwich C.T.	*a*	*b*	Angle E from N
			h m	m	m	°	h m	m	m	°
Occultations Visible in Longitude +72° 30′, Latitude +42° 30′										
Dec. 8	351 B.Aqar	6.5	23 0.0	..	..	137	23 3.7	..	..	141
11	*e* Pisc	5.7	5 33.0	..	..	349	5 51.2	..	..	316
12	29 Arie	6.1	23 21.5	−0.8	+2.1	46	0 36.9	−1.5	+1.5	242
18	*c* Gemi	5.4	10 2.7	−0.6	−2.0	122	11 4.5	−0.6	−1.4	275
19	28 Canc	6.1	1 19.1	+1.3	+4.2	28	1 40.4	−1.4	−2.3	344
Occultations Visible in Longitude +91° 0′, Latitude +40° 0′										
Dec. 8	351 B.Aqar	6.5	21 56.9	−1.9	+1.3	87	23 6.1	−0.9	+2.3	199
10	10 Ceti	6.4	5 28.2	−0.6	−0.2	57	6 34.2	−0.2	−0.3	240
11	*e* Pisc	5.7	5 19.3	..	..	350	5 47.6	..	..	305
11	*o* Pisc	4.5	22 28.3	..	..	141	22 33.9	..	..	151
12	29 Arie	6.1	23 8.8	0.0	+2.5	21	0 5.0	−1.5	+1.3	271
18	*c* Gemi	5.4	9 59.7	−0.2	−3.9	161	10 43.4	−2.3	+0.6	236
Occultations Visible in Longitude +98° 0′, Latitude +30° 0′										
Dec. 6	33 Capr	5.5	2 10.3	−0.5	+0.6	43	3 17.6	−0.6	−0.5	253
10	10 Ceti	6.4	5 27.3	−1.3	−1.1	88	6 27.3	−0.3	+1.5	204
11	*e* Pisc	5.7	4 47.9	−1.1	+1.9	29	5 59.4	−1.3	−0.7	260
12	29 Arie	6.1	22 45.6	+0.1	+2.2	30	23 42.9	−1.1	+1.3	263
Occultations Visible in Longitude +120° 0′, Latitude +36° 0′										
Dec. 5	40 B.Capr	6.2	0 53.7	−2.4	−0.7	92	2 5.7	−0.5	+0.7	215
10	10 Ceti	6.4	4 58.1	−1.1	+1.4	35	6 15.3	−1.3	−0.2	247

METEORS AND METEORITES

Meteor Notes from the American Meteor Society

By CHARLES P. OLIVIER, President

As is well known to all members of our Society, the Hydrographic Office of the U. S. Navy has cooperated actively for over twenty years in collecting data on fireballs and meteors. As a further example of their desire to aid such work, recently I was requested by them to write a brief article outlining why such observations have real scientific importance, and how they should be made by ships' officers and others. This was done, and the article appeared on September 25 in No. 3081 of the *Hydrographic Bulletin*. Anyone interested could obtain a copy from one of their offices, maintained in most coast cities, or directly from Washington. Of course, the methods of observations have long since been fully described in *A.M.S. Bulletins* 15 and 16, which have been furnished all members when they join, and to many others on request.

Articles are now appearing dealing with the fall of the Norton, Kansas, meteorites of 1948 February 18. It is understood that the largest piece exceeds in weight that of any other meteorite ever actually seen to fall. It is a stone.

As it receives full attention elsewhere, I should only like to remark that a long-enduring smoke train was left, and so far I have seen in print no attempt to derive its direction of drift, nor indeed any serious attempt to calculate the fireball's atmospheric path from which its orbit could be derived. All of this should be done at once, as it should be realized by everyone that such information is of comparable importance to that secured by the analysis of the meteorites themselves.

There now follows an account condensed from that of an eyewitness of the remarkable fireballs seen from northern Oregon, over Washington, on the night of 1948 August 17/18, and the solutions of the paths of two fireballs by Miss Nancy Weber and the writer.

Although *F. O. Reprint* No. 69 appeared less than a year ago, being a supplement to No. 60, and containing between them as complete a list of long-enduring meteor trains as the writer could compile, this subject is being followed here as intensively as ever. Therefore every report of a new train, or report of one in past years which we did not before know of, is eagerly placed in our files. Recently a phenomenon of this nature, unique to my knowledge, was reported to us by Mr. Harold L. Foreman of Irrigon, Oregon, who incidentally writes that he has long observed all fireballs seen and is hence familiar with them and also the constellations. His observations will be quoted at considerable length, as, for obvious reasons, we are most desirous of contacting others who saw these remarkable bodies. One other equally good report would permit us to calculate the heights, radiant, etc. ". . . August 18. They (7) fell out of the same region and died in the same region . . . left nearly parallel trails and fell only minutes apart so that there were several trails visible at the same time even after they had died." Time: first fireball at 2:14 A.M., train lasted until 2:20; second fireball at 2:16, train lasted until 2:23; five more meteors appeared by 2:38; no more seen after that though watched for. All started between A Cass. and γ Camel., and ended between θ Boo., and ζ Urs. Maj. The fireballs did not explode. He states that both were extremely brilliant, indeed he was awakened by the intense glare from the first and then rushed outdoors. The trains left were also very brilliant; they faded without appreciable drift or lateral motion. The second fireball, seen when he was outside, was so bright that it illuminated the landscape. The later five seem to have been bright enough to leave appreciable trains. He sent Map 1 with the meteors plotted thereon from memory, and there seems little doubt that the seven all belonged to a shower whose radiant had a right ascension of about 2^h, but unless we have other reports its declination cannot be derived. But that two extremely brilliant and long-enduring trains, 3° apart only, should be visible at once, is unique. As they were seen in the north from Irrigon, Oregon, which is on the Columbia River, and is not far from midway in longitude between Spokane and Seattle, the objects might have been seen low in the west, and low in the east, respectively, from these two cities. The country in south central Washington is sparsely populated and hence reports would, at best, be few for such an hour of the night from the region which they actually traversed from east to west. But such reports are urgently needed! If anyone, reading this, could put me in touch with another eye-witness, I would be most grateful.

As the Moon will be full very near the maxima of both the Orionids and Leonids, few meteors from either radiant will probably be seen. Also the main group of the Leonids is now pretty close to their aphelion point, so few of them need be expected for some years to come. Only for the Geminids will the Moon

set in time, on maximum night, for there to be a few hours in which the meteors may be observed without its interference, and these late in the night.

Fireball of 1947 November 21/22

By Nancy E. Weber and Charles P. Olivier

On this date a splendid fireball appeared over western Oregon, traveling from south to north. Thanks to the excellent organization effected by Professor J. Hugh Pruett, our regional director for that area, he was able to secure a large number of reports (about 50). These consist of both original letters, and filled-in questionnaires sent out by him to the presumed better observers. All these were later sent to A.M.S. headquarters, and here we were able to add another observation from a ship, station (50), printed in the *Hydrographic Bulletin*, U.S.N.

Following our usual procedure, a map with converging meridians was made to scale, and the observers' stations plotted. The azimuth lines were then laid down, and the center of the areas of their converging lines sought. From nine stations observers report the path perpendicular, and two report it nearly so. But those to the side saw it coming down at a considerable angle. Unfortunately, only from the ship (50), λ 124° 18′, ϕ 45° 08′, do we have an altitude and azimuth for the beginning point which can be checked, as the observer saw it very near Procyon. We find his coordinates correct. For some unknown reason (50) gives the end altitude but not the azimuth. Nevertheless, due to the check, we were forced to make this the key observation for the solution. No one else used an identifiable celestial body to aid the estimates. However, we were obliged to lay off the path so that it would be as nearly perpendicular as possible for the stations so reporting it. This was well accomplished for: (6) three miles west of Odell Lake, (7) Wicopee, (24) Grants Pass, (25) Gresham, (26) Portland, (37) Klamath Falls, (38) Sprague River, and (36) Castle Rock; fairly well for (22) Parkdale, and (31) Yamhill; and poorly for Dallas.

We could use only nine altitudes for the height of the beginning point, two for intermediate points, and 15 for end heights. The value for H_1 was 144 ± 33 km., but when the two intermediate points were allowed for, this was lowered to 140 km. The end height H_2 is 62 ± 19 km.

Practically everyone speaks of an intense bluish-green color, even people indoors who only saw the light, not the fireball itself. It exploded or flared up greatly at the end. The intensity of the light must have far exceeded that of the full Moon. No long-enduring train was left. Leaving out two excessively long estimates of duration we have 14 reports ranging from two to ten seconds: mean $4^s.51 \pm 1.92$. If the two next highest—almost surely wrong!—were left out, 12 estimates would give $3^s.77$. No sound from the explosion was reported. The end height seems too great for meteorites to be expected.

The one report in which we may have full confidence, due to checks by a brilliant star, gives for H_1 184 km., for H_2 84 km. In each case these are *higher* than the adopted values given by the full mean deviations. This illustrates the danger in assuming that observers over-estimate their altitudes (as indeed so frequently is true!) and then to arbitrarily attempt to correct them. It is our practice here to use altitudes as given, only omiting those, in general, over 60°, for from then on estimates become very poor. Altitude estimates in western Oregon also must often suffer from a mountainous horizon, as few would know how to allow for this accurately.

The tabular data follow.

Date	1947 November 22.38 G.C.T., 1947 November 21.88 G.M.T.
Sidereal Time at End Point	75° 17′
Began over	$\lambda = 122°\ 7'$, $\phi = 43°\ 46'$; height 140 km
Ended over	$\lambda = 122°\ 14'$, $\phi = 44°\ 32'$; height 62 km
Length of path	116 km
Projected length of path	86 km
Duration	3ˢ.77
Observed velocity (uncertain)	30.8 km/sec
Radiant (uncorrected)	$a = 353°.5$, $h = 42°.5$
Zenith correction (parabolic)	—0° 57′
Radiant (corrected)	$a = 353°.5$, $h = 41.6$; $\alpha = 80°\ 9'$, $\delta = -3°\ 40'$

The radiant is A.M.S. No. 2313.

The elements of the parabolic orbit are as follows:

$$i = 141°$$
$$\Omega = 59°$$
$$\pi = 158°$$
$$q = 0.421 \text{ A.U.}$$

The work on this fireball was divided as follows: Pruett collected and partly digested all the data, Miss Weber drew the map and did most of the computing, including the orbit, Olivier examined the data critically and chose the subpoints after several approximations, and worked out the atmospheric path. No radiant in this position and for this date is found in the von Niessl-Hoffmeister Catalogue.

Our sincere thanks are due to the fifty or more persons whose reports made this solution possible. It is, from want of space, obviously impossible to give the individual names and observations. Our work was carried out under a grant from the Faculty Research Fund of the University of Pennsylvania.

Fireball of 1948 March 4/5

By Charles P. Olivier and Nancy E. Weber

On this date at 9:10 P.M., P.S.T., a fine fireball, moving almost directly from south to north, appeared over the Pacific, off the coast of Oregon. Professor J. Hugh Pruett of Eugene, Oregon, at once began the collection of reports on this object, later sending questionnaires to the more promising observers. He worked out and published in local papers preliminary heights, etc., and then sent the data to A.M.S. headquarters. Here, following the procedure outlined in several recent papers in these Notes, a solution was made. In this case we are most unfortunate that no ship in the area sent in a report though the fireball must have passed in sight of many. A single good observation from under or west of the path would have added much to the accuracy of the solution.

Approximations were made here for no less than four sub-beginning and five sub-ending points. Those finally chosen depend upon a larger percent of the total number of reports than is usual. Only one observer, (22) at Seattle, referred her beginning point to an identifiable star in Orion. She also stated that the path was horizontal. Her estimated altitudes were 30°: from her statement about β Orionis we calculate they should be 24°. No one else gave any means for checking the altitude estimates. Only one station, (10) Lighthouse Way, Coos Co., gives such small differences in azimuth that the path must have been nearly vertical. Altogether, seven altitudes were used to determine directly the begin-

ning point, which turns out to be 142 ± 20 km. Fourteen altitudes were used for the end height, and two for intermediate points. Using these two intermediate ones as a partial correction to H_1, this was raised to 145 km. The average deviation for H_2 is excessively large yet a close study of the data does not permit us to discard one of the values more than another. Nevertheless, it is believed that the slope is about correct, though indeed the whole calculated path is systematically too high as is shown by (22), the one altitude we can check. To indicate, however, the uncertainty attendant upon any arbitrarily attempted correction to altitudes, if we bring the adopted mean of all to the values given from (22) alone, we would have to drop the path by 45 km. On the contrary, if we correct all by that shown by the 6° difference in the estimated altitude at (22) and that calculated by means of Rigel, we would drop the path by only 20 km. Such uncertainties are most regrettable but are often unavoidable when stars, planets, or Moon are not used by the observers. Nothing we can do to the observations of this fireball would change the path from an almost horizonal course, and one quite high in the atmosphere.

The duration of flight based upon 20 estimates ranging from 2.5 sec. to 12.5 sec. as extremes (one of 30 sec. was omitted as wholly impossible; the observation was otherwise poor also) was 5.72 ± 1.92 sec. The estimates of color differ widely, due probably to the short trail being often mixed up with the body itself. A total of 13 observers out of 20 use the word "blue" among others, and seven use "white," some of these observers are the same. We assume therefore that it was bluish-white in color, with a probable short yellow train, which did not persist. It is doubtful that it exploded or greatly flared at the end. Estimates of size vary from that of Venus to the full Moon. Ten observers give angular diameters, by referring to the Moon. The mean of these ten would be 14′. At any rate, it seems that the body was large enough to show a very appreciable disk. The table of data follows.

Date	1948 March 5.22 G.C.T., 1948 March 4.72 G.M.T.
Sidereal time at end point	115° 02′
Began over	λ 124° 54′, φ +45° 55′; height 145 km.
Ended over	λ 125° 20′, φ +47° 58′; height 140.8 ± 44.6 km.
Length of path	230.6 km.
Projected length of path	228.9 km.
Duration	5.72 ± 1.92 sec.
Velocity (uncertain)	40.3 km/sec.
Radiant (uncorrected)	Azimuth 352°.5, altitude 2°.4
Zenith correction (parabolic)	—5°.2
Radiant (corrected)	Azimuth = 352°.5, z = 92°.8; α = 125°.5, δ = 44°.4

The radiant is A.M.S. No. 2314.

The elements of the parabolic orbit are as follows:

$$i = 34°$$
$$\pi = 312°$$
$$\Omega = 165°$$
$$q = 0.91 \text{ A.U.}$$

As always, we are under many obligations to the persons, not mentioned individually for want of space, who so kindly sent in reports and filled in questionnaires for Professor Pruett, our regional director in that part of the country. Without this help from persons willing to aid science, such solutions would be impossible. The very large part of the work done here by Miss Weber was made possible due to a grant from the Faculty Research Committee of the University

of Pennsylvania. No radiant in the von Niessel-Hoffmeister Catalogue is found near that of this fireball for this date.

Flower Observatory of the University of Pennsylvania, Upper Darby, Pa., 1948 October 9.

Contributions of The Meteoritical Society

(Known Formerly as The Society for Research on Meteorites)

Edited by FREDERICK C. LEONARD
Department of Astronomy, University of California, Los Angeles 24

The Non-circularity of the Canyon Diablo, Arizona, Meteorite Crater*

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ABSTRACT

In an attempt to obtain photographic evidence relative to the existence or non-existence of "rays" in connection with the Canyon Diablo, Arizona, Meteorite Crater, aerial photographs were secured, which showed the Crater to be distinctly non-circular in outline.

On several occasions, reports have been brought to my attention that flyers have observed "rays" in connection with the Canyon Diablo, Arizona, Meteorite Crater, similar to those radiating from certain lunar craters. Altho the self-styled observers of these "rays" had no known scientific training, and were in all cases merely lay visitors at the Griffith Observatory and Planetarium in Los Angeles, I felt it worth while to make a flight over the Crater, in order to investigate the contention, even tho the probability of finding anything of the kind seemed to be exceedingly remote. The stories regarding the so-called "rays" all agreed on 2 points, namely, that, in order to see these features, the observer must be at an elevation of at least 12,000 feet above sea-level, and that the observations must be made at or very near noon. (The fact that the lectures at the Griffith Planetarium stress that lunar rays can be seen only near the phase of full Moon may have been a factor in the latter point of agreement.) In order to reproduce these conditions, I made several photographs of the Crater, on 1948 May 30, nearly at noon, local time, and at an indicated pressure-altitude of about 13,000 feet. I also circled the Crater several times, trying to make out anything that resembled what had been reported. It is sufficient to say that I saw nothing of the nature of "rays," and that later, natural color-photographs confirmed the visual observations.

When these color-photographs, together with others taken after landing, were shown to Dr. Frederick C. Leonard, he at once remarked that he had not before realized that the Crater was as non-circular in outline as the pictures indicated, and he wondered whether some factor of distortion caused the squarish appearance. Altho we could see no reason for any distortion, I decided to make

*Read at the 11th Meeting of the Society, Albuquerque, New Mexico, 1948 Sept. 7-8.

FIGURE 1

AN AERIAL PHOTOGRAPH OF THE CANYON DIABLO, ARIZONA, METEORITE CRATER, TAKEN FROM AN ALTITUDE OF 13,600 FEET ABOVE SEA-LEVEL, ON 1948 OCTOBER 2, BY WALTER W. ZIMMERMAN

(North is at the Top of the Photograph.)

a second trip, and to take a number of photographs from as nearly vertical a position over the center of the Crater as possible.

On 1948 August 1, I made 10 photographs on 35-mm., Kodachrome film with a Leica camera, using a variety of exposure times and aperture settings. Because of the extreme turbulence of the air, and the presence of a fractocumulus layer immediately above, it was difficult to maintain a uniform altitude for all

the exposures, but they were all made between the limits of 13,900 and 14,700 feet, as indicated by the pressure-altitude, and from points located as nearly vertically over the center of the Crater as possible.

All the resulting natural color-transparencies show that the Crater is certainly not circular, but is best described as being roughly squarish, or as being a square with rounded corners. The quadrilateral or octagonal character is apparent when it is noticed that a considerable segment of the east and west rims is nearly straight, as is also that of the north and south rims, altho to a lesser degree. The central parts of the east and west rims, which are nearly parallel to each other, as well as being almost straight, further accentuate the non-circular appearance of the Crater.

It is probable that other observers have noted that the outline of the Canyon Diablo Crater deviates from a circle, but it seems appropriate to record the circumstance here, in a more formal manner, in view of the fact that the non-circularity of the Crater proved to be a real surprise to at least 2 members of The Meteoritical Society—namely, Dr. Leonard and me!

1948 September 4

Streak Meteors without Perceptible Nuclei*

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ABSTRACT

The phenomena of streak meteors without perceptible nuclei are described, and a possible explanation of the phenomena is given.

Sometimes a meteor without a perceptible nucleus is "felt," rather than seen, to pass across the sky. Generally, immediately afterwards (but more rarely after a second or two), a fairly bright, broad streak follows in the wake of the meteor, about 3° or 4° long and 1° or 2° broad, with a color resembling that of the cathode glow in a vacuum discharge-tube. The magnitude of this type of meteor borders generally on 2.5, and the streak lasts for a couple of seconds; occasionally the meteor is brighter, and the streak lasts for a longer time without perceptible drift. The phenomenon—as far as it is observed by the unaided eye—may be ascribed to the motion of a tiny piece of cosmic matter surrounded by an imperceptible "gas cap." The fact that the streak is confined to a short length naturally leads to the conclusion that the apparition is limited to a particular region of the atmosphere (*v.* a "Letter to the Editor" of *Nature*, **138**, No. 3500, 933, 1936, on the "Luminosity of Meteoric Trains").

Years of observation (adequate allowance having been made for changes of meteorological conditions *&c.*) have convinced me that such meteors form a class by themselves. Their streaks are due to the ionization of air atoms, whereas the streaks accompanying large fireballs, on the other hand, consist mostly of glowing matter detached from the large incandescent meteorites themselves. Very often, as has been pointed out in several synopses of meteor observations at Begumpet (Hyderabad Academy Studies), even apparently slow-moving meteors

*Read at the 11th Meeting of the Society, Albuquerque, New Mexico, 1948 Sept. 7-8.

exhibit bright streaks, whereas fast or brighter meteors (of even the first or the zeroth magnitude) are devoid of them.

Streak meteors without perceptible nuclei are well worth studying, both photographically and with the aid of shutter-regulated cameras. It seems justifiable to conclude that these streak meteors are caused by tiny particles of cosmic matter moving with fairly high velocities in particular upper regions of the air, but that the ionization they produce in the air is incomparably greater than what would be expected from their mass or size. It would be too rash at present to associate these phenomena with "contraterrene" matter, which is, of course, physically possible (and perhaps even necessary, if the material universe is supposed to have derived from all-pervading energy alone), as more tangible proofs of the existence of this kind of matter are necessary before it can be accepted as a reality. A more plausible explanation seems to be the following: neutrons from cosmic rays, in the appropriate regions of the air, excite radioactivity in the body of the incandescent meteorite and its vapor, which, in turn, copiously ionizes the air in the path of the meteorite. Cosmic rays are believed to produce about a dozen neutrons per square foot per second.

1948 January 18

A Reply to an Article by Mr. Brandon Barringer on the Origin of the "Carolina Bays"*

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ABSTRACT

The 5 points of criticism raised by Mr. Brandon Barringer against the probable meteoritic origin of the "Carolina Bays" are each briefly discussed, and the conclusion is reached that these objections are not valid under the existing conditions and under the modified meteoritic theory of bay origin.

In *C.M.S.*, **4**, 33-5; *P. A.*, **55**, 215-17, Apr., 1947, was published an article by Mr. Brandon Barringer giving a number of reasons why that author thought it improbable that the so-called "Carolina Bays" were formed by meteoritic action. These reasons were as follows:

(1) No surface meteorites have been found directly associated with any of the bays.
(2) The strata beneath the rims of the bays are undisturbed.
(3) Small bays should have been filled in more readily in North Carolina than the big bays, as they were at Odessa, Texas.
(4) Lineated bays, such as those near Rains, South Carolina, seemingly would not be formed by "a chance distribution of a swarm of masses [meteoritic] in space."
(5) The magnetic high found at the southeast rim of one of the bays, when the dip needle was used, seemed too low to be an argument for the meteoritic origin of the bay.

I should like to discuss the foregoing objections in the order given.

(1) The age of the Carolina Bays is thought to be late glacial. The time is roughly estimated as 20,000 years ago. The Carolina Bays are confined to the

*Read at the 11th Meeting of the Society, Albuquerque, New Mexico, 1948 Sept. 7-8.

sandy Coastal Plain. The area of most abundant meteorite finds in the United States, just to the northwest of the area of the Carolina Bays in the Piedmont, is characterized by clay soils, where shallow meteorite scars would soon be destroyed by erosion and sedimentation and where meteoritic masses, buried in the clay soil, would have a much better chance for preservation than they would, buried in the loose-textured, sandy soil of the Coastal Plain, where water with organic acids circulates freely. It does not seem reasonable to assume that any ferruginous material which rested in the near-surface layers of the Atlantic Coastal Plain sands could long withstand decomposition and solution; on the other hand, erosion of the sand rims by rain water is almost negligible, and erosion by wind is at a minimum in the moist climate.

(2) If we assume that the bay depressions and sand rims were formed *chiefly* by the wind-shock-waves,[2,3,4,5] accompanying the infall of meteoritic masses, and not by the splash and explosion effects, which should have been confined to a small area a little forward of the center of the bay, we should expect to find undisturbed ground, not only under the rims but also in much of the bay area. The force of the wind-shock-waves should have been sufficient to make large, shallow, elliptical, sand-rimmed depressions.[1a] Small-scale, elliptically rimmed depressions, similar to the bays, can be readily made by the compressed-air cone accompanying high-speed bullets shot into rock powder.[4,5]

(3) Many of the small bays, as well as many of the large bays, are now under cultivation. Such have been naturally or artificially drained. Some bays are partially filled with dune sand, but the chances for the small bays to be filled, in the moist climate of the Carolinas, is not so great as it is in the Texas area of less rainfall.

(4) Lineated bays, illustrated by the group of 6 bays in the article by Barringer, are not unusual. Lineation may occur in various directions for no apparent local petrographic, structural, or topographic reason. In some cases, lineation of "multiple bays" is explained by tandem meteorites hitting a rotating Earth.

(5) The inconclusive magnetic reading at the southeast rim of the Carolina Bays is the usual condition. The magnetic high associated with the bays is at a considerable distance to the south of the southeast end of the bays.[2,3,4,5,6] One would expect the magnetic bodies associated with the Carolina Bays to be at a relatively greater distance from the southeast end of the bays than from the forward end of the Canyon Diablo, Arizona, Meteorite Crater, because of the greater ellipticity of the Carolina Bays.

On close analysis of Mr. Barringer's 5 objections to the meteoritic origin of the Carolina Bays, only one objection seems valid, namely No. 2, which holds that there must be disturbed strata beneath or close to the sand rims of a meteorite crater, as pointed out by the original meteoritic theory.[1] Under the modified meteoritic, or wind-shock-wave meteoritic, theory, several times referred to in the literature on the origin of the bays,[2,3,4,5,6] objection No. 2 is satisfactorily explained.

After more than 14 years of study and field work in the area of the Carolina Bays, the author believes all present-known facts concerning the Carolina Bays to be logically explained by the Wind-Shock-Wave Meteoritic Theory. The author expects to publish within the coming year a detailed map and report dealing with the Carolina Bays. This report will discuss in considerable detail the known facts about the bays, the results of the magnetometer surveys, and the various theories of their origin.

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Comet Notes

By G. VAN BIESBROECK

During the last month the orbits of the recently discovered comets, ASHBROOK (1948 *i*) and JOHNSON (1948 *j*), have been determined so that their future course can now be predicted. The first one proved to have a short period, returning after 7½ years. Two independent computations give practically the same result.

ELEMENTS OF COMET 1948 *i*

Computer	L. E. Cunningham	A. D. Maxwell
Perihelion time	1948 Oct. 7.98083	Oct. 6.73313
Node to perihelion	350°03525	349°56073
Longitude of node	2.18630 } 1948	2.32782 } 1948
Inclination	12.62167	12.49722
Semi-major axis	3.8469765 A. U.	3.8240859 A. U.
Eccentricity	0.3975056	0.3966061
Period	7.5455 years	7.4732 years

The comet is evidently a large one to be as bright as 11th magnitude at a perihelion distance of 2.32 astronomical units. The small inclination will keep this object near the ecliptic and therefore in reach most of the time. At aphelion the comet moves a little beyond the orbit of Jupiter. In fact Cunningham remarks that in 1945 it was within 0.3 astronomical units of that major planet and that the orbit may have been modified considerably at that time. From the distance relation the theoretical brightness should not vary by more than 6 magnitudes between oppositions at the two ends of the major axis and this makes it probable that with the larger instruments it will be possible to follow this comet all around its orbit.

The following ephemeris deduced from the first set of elements will enable observers to follow the comet as it moves from Aquarius into Pisces while slowly losing in brightness.

EPHEMERIS OF COMET 1941 *i*

1948	R.A.	Dec.	Distance: From the sun	Distance: From the earth	Magnitude
	h m	° ′			
Nov. 1	22 41.4	−8 58	1.663	2.324	11.2
9	44.9	7 46	.753	.329	.3
17	50.0	6 29	.849	.335	.4
25	22 56.4	5 8	1.949	.343	.6
Dec. 3	23 3.9	3 44	2.053	.352	.7
11	12.5	2 17	.159	.362	.8
19	21.9	−0 47	.266	.373	11.9
27	23 32.1	+0 45	2.374	2.386	12.1

The faint comet, JOHNSON (1948 *j*) is situated too far south for most observers on this side of the equator to be of interest. I recorded it with the 82-inch reflector of the McDonald Observatory as a small round coma centrally condensed and not brighter than 14th magnitude. Preliminary elements by Naur (Copenhagen) have been announced as follows:

ELEMENTS OF COMET 1948 *j*

Perihelion time	1949 March 9.776	
Node to perihelion	186°44′	1948
Longitude of node	138 10	1948
Inclination	51 0	1948
Perihelion distance	4.4388 A. U.	

While this parabolic orbit may have to be modified, it shows that this is an extremely distant object and that it will remain very faint.

Nearly a dozen previously announced comets have been kept under observation this last month but all of them are too faint for ordinary telescopes. The only one that will still be in reach of medium-sized instruments is COMET PAJDUSAKOVA-MRKOS (1948 *d*) which comes within less than three degrees of the north pole in the middle of November as shown by the following ephemeris deduced from improved elements by L. E. Cunningham.

EPHEMERIS OF COMET 1948 *d*

1948	R.A.	Dec.	Mag.
	h m	° ′	
Nov. 1	10 7.6	+85 16	11.8
9	7 15.8	87 22	.8
17	3 14.3	86 2	11.9
25	2 2.1	82 31	12.0
Dec. 3	1 41.8	78 30	.1
11	1 36.4	74 19	.3
19	1 37.0	70 9	.4
27	1 40.6	66 8	12.6.

McDonald Observatory, Fort Davis, Texas, Oct. 11, 1948.

Communications and Comments

Under this heading we shall publish from time to time such material as does not properly fall under any of the established headings of this journal. Here, too, may be found, when occasion arises, articles which the editor may not be willing to give sanction to but which, nevertheless, may be provocative of thought along new lines.

On the Advancement of Science

Looking backward over the years, we sometimes find it hard to understand why the men of science have been so slow in accepting ideas that now seem obvious.

A. Pannekoek, writing in POPULAR ASTRONOMY, January, 1948, "The Planetary Theory of Copernicus," tells of the trials and tribulations this early scientist had in evolving his new theory that the earth revolved around the sun, and in breaking away from the older Greek theory that the earth was the center of the Universe. Pannekoek seems disappointed by the failure of Copernicus to break away from Greek tradition, for he says that in the latter half of his book, "De Revolutionibus," Copernicus was in almost complete agreement with Aristotelian philosophy. Pannekoek writes: "And when then, we dive into the further chapters of this revolutionizing book we feel ourselves transferred entirely into the sphere of antiquity. In every detail, in every next chapter, it tries to cling almost anxiously to the time-honored example of old Ptolemy. Nowhere the breath of a new time is felt. Nowhere the trace of the daybreak of a new epoch of scientific research."

How different, we ask, were Copernicus and men of his day from modern scientists and laymen who take for granted certain "time honored" theories in other branches of science for which they are not qualified by training?

In a recently published textbook on astronomy, a statment is made concerning the age of the sun which illustrates this point. The textbook states that the astronomer has no way of finding the age of the sun except by going to the geologist who has found the age of the earth to be at least 2 billion years. Since it is believed that the earth and other planets were thrown off from the sun, the sun must be older. The geologist, on the other hand, has already visited the astronomer to learn about the origin of the earth. With the beginning furnished by the astronomer, he proceeds to determine the age of the earth by a study of the physical evidence available. One method has been to study the rate of erosion or sedimentary deposit and from this known factor determine the age of the earth by the depth of sedimentary rocks that have been laid down one upon another. A more recent method has been to measure the rate at which radioactive elements disintegrate or change into other elements. Then by measuring the amount of radioactive material remaining in the very ancient rocks, the age of the earth is determined.

The questions then arise: Are we sure that the rate of sedimentary deposit has always been approximately the same? Are we even sure that the radioactive elements have always had the same rate of change? These questions point to the necessity of first determining the cause of change in the earth's crust. We cannot determine these factors by simply taking a sample of this rate of change as it is today. We might as well try to determine the age of a man by his rate of growth

or degeneration of body over a day's time in his life. By this method a man from Mars might have difficulty in determining the age of an earth-man specimen, if he should capture one. In other words a few thousand years of recorded history, or perhaps a hundred years of recorded scientific research are not enough to prove anything about the rate of growth of the earth.

Apparently, all things in nature, from the greatest to the smallest, follow the same pattern of birth, growth, maturity, and decay. The earth is no exception. It follows then, that we must first learn everything possible about the origin and growth of the earth before we begin devising elaborate time charts.

Why is man so interested in fixing an age on everything under the sun, anyhow? What is the psychology behind it? This time element lurks behind almost every page in all the volumes written on geology, paleontology, and anthropology. Why must we fix the age of every stratum of rock and the fossils found therein? Is it important whether Neanderthal man or Java man is older? If it is important, *then we must first decide whether the earth has undergone sudden and violent catastrophic change or if its past is only a record of slow uniform evolution.* Most geologists will say that Uniformity and Evolution are established facts and that the cataclysmic theory has been dead for over a hundred years. However, it could be that they are only venerating long established theory as Copernicus did, and too, some may have a subconscious religious phobia.

Probably one of the greatest detriments to the advancement of the science of geology, has been the long quarrel between this branch of science and religion. Geology has had to bear the brunt of the religious attack because of the differences of opinion about the origin of the earth, evolution, creation, etc. The more modern branches of science which were not known to the ancients and which were not mentioned in the Bible, have been left comparatively free from this necessity of fighting religious opposition. As everyone knows, Fundamentalism *versus* Evolution has been a common topic for debate and name-calling-argument for the last hundred years, and in some cases it was a life and death matter. As a result, open-mindedness is not too common on either side, even today. Such a wall of resistance has been built up in the minds of geologists, that they automatically reject any idea that smacks of religious teaching, such as cataclysmic floods, even though a real physical cause is given for these floods. They see a religious bogey behind any such idea.

Because of the ill-will generated by this long and tiresome quarrel, some geologists have lost sight of their goal and spent a great part of their energy in trying to disprove the Bible rather than making a dispassionate study of the facts before them. They will be loathe to admit this, and no doubt few have even realized it, but their efforts to prove how old the earth is and to lay out a very intricate time chart of epochs and ages are, in reality, an effort to disprove the arguments of their religious opponents. Each generation of geologists has inherited this old quarrel from its teachers and the necessity for fighting for a great truth has been kept alive. In their efforts to disprove the Bible, some geologists have assembled a vast array of facts (well mixed with theory) based on the postulates of Uniformity and Evolution. This great mass of material has been so hallowed by time and by the "holy wars" fought in its defense, that its advocates, without knowing it, have become as self-righteous and dogmatic as their religious adversaries. Thus a fight for a scientific cause has resulted in a

lack of vision in their own thinking. It is not strange then, that there has been nothing basically new added to the science of geology since Sir Charles Lyell advanced the theory of Uniformity in his "Principles of Geology" in 1833. The principles of Uniformity and Evolution are still the basic foundations upon which geology is laid.

Perhaps the best reason why this quarrel has been kept alive so long is the fact that the earth is literally covered with evidence of a great flood which must have occurred in very recent times and this ever-present ghost still haunts them (the geologists). This evidence was investigated and discussed by many of the early geologists. Some attributed it to the Bible Flood while others thought that sudden upheavals or sinking of the earth's crust had left this evidence of oceanic flood. More recently, in 1923, an experienced geologist, George McCready Price, wrote a book called "the New Geology" in which he points out a great many cases in which Uniformitarian Geology either has no answer or is in very grave doubt. Price believed in an hypothesis of sudden world catastrophe caused by some "outside force" which upset the oceans and caused the universal deluge recorded in the Bible and in the legends of many peoples. He did not say that God created this "outside force." He does not say what it was, but his critics have read between the lines, and knowing that he was a religious man, they have ignored his work completely. There was not much else they could do. Thus the quarrel continues in a sort of "underground" fashion and the advancement of science is retared by a backstage fight, while the real problem, the basic cause of change in the earth's crust, is forgotten.

It is my endeavor to get scientists (geologists and astronomers in particular) to reconsider the old theory of cataclysmic deluge in the light of present day scientific knowledge. To cast aside all prejudice and to look for evidence of every kind indicating that the earth has been bombarded by cosmic bodies of large size in past ages. It is already a locally accepted theory that the moon has been subjected to such bombardment. Is there then, any scientific reason to suppose that the earth could have avoided similar treatment? Is there any scientific reason why the entire history of the earth should be predicated on the uniformity of conditions prevailing over the last few thousand years of recorded history?

There are some signs that scientists are beginning to question the theory of Uniformity. A few men with important names in the field of astronomy have privately indicated their belief in the collision theory. An article appeared in the *Los Angeles Times* of March 7, 1948, headed: "Geologists Map Attack on Minerals Shortage" and subhead: "Strategy Evolved for Applying Manpower and Money to 10 Projects Labeled 'Urgent'." This was under the direction of Dr. Joseph L. Gillson of the E. J. du Pont de Nemours Corporation and Dr. Richard H. Johns of Caltech. Of these ten projects the third was, "Sediment studies seeking a fuller understanding of the *'beginnings** of deposits laid down by water, wind, and ice'." It is becoming obvious that a more thorough knowledge of the *basic causes of change in the earth's crust beginnings* are very important if we are in urgent need of discovering new mineral resources.

Astronomers are certainly aware of the fact that cosmic bodies may collide with the earth, although little publicity is given it because of the wild stories

*The italicizing is mine.

that might appear in the press, scaring the wits out of many people. As an example: Harvard College Observatory sent out the following announcement card 877. Fast Moving Object.—Dr. V. M. Slipher, Lowell Observatory, Flagstaff, Arizona, writes. "A fast moving object of magnitude 13 was recorded on plates made Dec. 12 and 14, 1947, by Mr. H. L. Giclas. The plates were not examined until ten days later, and all attempts to relocate and confirm the object in the bright moonlight at that time and subsequently have been unsuccessful. The trails on the 90 minute exposures, taken exactly two days apart, are 5′ 24″ and 6′ 2″ long respectively on the two dates. The trails, except for length, appear very similar to a minor planet trail of comparable brightness." Dr. L. E. Cunningham of the University of California calculated the probable orbit. This is said to have indicated a velocity so high that the body would be shooting through the solar system from outer space and that it passed within 25 million miles of the earth. Probably the nearest miss on record for an object of this size.

Another significant straw in the collision wind: Dr. Harrison Brown of the University of Chicago, won the $1000 prize for the most "notable contribution to science," in a paper read before a meeting of the American Association for the Advancement of Science in Chicago, December, 1947. Dr. Brown's paper, "Elements in Meteorites and the Earth's Origin," was selected from over 2000 papers by the Prize Committee. Part of this paper (as reported in *Time Magazine*) had to do with estimating the size and mass of a planet that is thought to have once revolved in an orbit between Mars and Jupiter. Astronomers have suggested that this planet may have collided with some other object, disintegrating into the thousands of asteroids that occupy this orbit today.

And so we catch a glimpse of the slow progress of science as it was in the time of Copernicus and as it is today. Scientists still venerate the great men of the past and they still criticize and quarrel with one another and with lesser folk. Will evolution some day produce the perfect man, or will man's Atomic Bomb or some visitor from outer space, start the process all over again?

ALLAN O. KELLY.

Carlsbad, California, March, 1948.

Whistling Meteors

Almost all of us, at one time or another, have gazed in awe at the heavens and on into outer space. Therefore, we are probably familiar with the shooting stars or meteors which are constantly trying to penetrate the earth's atmosphere. These meteors are truly called the driftwood of outer space since they consist of particles of matter probably left over when this universe was made. The ordinary meteor, which one sees on a clear, summer night, is only about the size of a pea. Although larger ones do occur, as evidenced by the huge crater in Arizona, they are very rare. It has been estimated that in twenty-four hours over eight billion meteors strike the earth.

Few of us are aware that it is possible to "hear" these visitors from outer space during their short journey through our atmosphere. The only equipment required to "listen in" on them is a good short-wave receiver. The use of radio to detect meteors, as they hurtle through the ionsphere, makes use of one of the most important properties of short radio waves, namely their ability to be reflected.

When meteors are streaking through our atmosphere at a speed of almost 30 miles per second, it is small wonder that the collision of these objects with the molecules of gas comprising the atmosphere releases an enormous amount of heat energy. These particles become incandescent, somewhat like the gas in a neon sign. The resulting agitation causes intense ionization along the path of the meteor. Almost ninety per cent of the energy in a meteor is used to produce ionization, the rest being spent in friction. This ionization rapidly diffuses outward after the passage of the meteor. Eventually the ionization is dissipated and the reflecting region disappears. A meteor of average speed weighing only one quarter of a gram would ionize a portion of the atmosphere one mile across and one hundred miles long. This same ionization that forms the glowing tail of the meteor also has the remarkable property of reflecting radio waves, much the same as they are reflected from regular layers of the ionosphere. It is this fact which makes it possible to detect their presence even in bright day light.

In order to hear the howls and whistles which are the characteristic sounds made by meteors during their plunge through the atmosphere, it is necessary to tune in a powerful short-wave broadcasting station preferably in the twenty-five or thirty-one megacycle bands and from sixty to one hundred miles away. The main requisite is that you should not be receiving a direct signal from the station. It must be very weak and fading and have a hollow sound. If this is the case, you will then be receiving a signal which has been "scattered" back to the receiver by means of various irregularities in the earth's surface.

Now suppose that a meteor enters the ionosphere headed directly toward the transmitter and receiver. If conditions are right, an irregular burst of strength will be noticed in the received signal. This burst will probably last from ten to twenty seconds and the signal will soon fade down again to its previous strength. It should also be noted that during the first part of the burst an eerie howl or whistle is heard on the carrier of the broadcasting station, lasting for only two or three seconds and varying in pitch.

Since a meteor trail moves through space with the same velocity as the meteor itself, the signal reflected by the front edge of the trail reaches the receiver by a path rapidly varying in length. The effect is the same as if the transmitter were located on the meteor itself. As a result, the varying distance between the meteor and receiver produces a "Doppler shift" in the frequency of the signal. This is similar to the manner in which the motion of an approaching locomotive changes the pitch of its whistle. Part of the signal arriving at the receiving location consists of energy reaching the receiver by means of a path of constant length. The rest consists of energy rebounding from a moving meteor trail. The difference in frequency, or pitch, of these two components produces a beat note easily detected at the receiver. It is this audio note which accounts for the characteristic howl or whistle which occurs at the beginning of the burst of signal strength in the receiver.

This method of detecting meteors is actually a practical example of the principle of radar. Instead of using microwaves, ordinary short waves are used, and instead of ships and planes, meteors are detected as they bore into the ionosphere from outer space.

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General Notes

Dr. A. E. Whitford has been appointed Director of the Washburn Observatory at the University of Wisconsin. He succeeds Joel Stebbins who retired on July 1.

Dr. Frederick C. Leonard, Professor of Astronomy in the University of California, Los Angeles, was in residence, from August 7 to September 9, 1948, at the Institute of Meteoritics of the University of New Mexico, of which he is a Research Associate. From August 17 to 26, he was a member of the I.O.M. expedition that was mainly responsible for the recovery of the 1-ton Furnas County, Nebraska, achondrite. This specimen, which fell on 1948 February 18, is the largest aerolite (stony meteorite) on record and the largest meteorite of any type ever observed to fall. (An article by Dr. Leonard on the Furnas County meteorites appeared in *C.M.S.* in the October issue of *P. A.*)

National Capital Astronomers is an active organization of amateur astronomers in Washington, D. C. This organization publishes a monthly pamphlet, entitled "Stardust," describing its interests. It holds numerous meetings, some for observation, some for discussions, and some for formal lectures. The current number of "Stardust" includes the list of members. The number at present is more than 120. Among the names are some which appear frequently in the astronomical literature.

The National Council of Teachers of Mathematics will hold its ninth Christmas Conference at Ohio State University, Columbus, Ohio, on Wednesday and Thursday, December 29 and 30, 1948. To meet the needs of teachers whose interests vary from the elementary grade to the college level, six sectional meetings will be held in addition to the general sessions. Headquarters for the National Council will be in Baker Hall on the University Campus. Reservations for rooms and meals should be mailed to Mr. Oscar Schaaf, Room 120, Arps Hall, Ohio State University, Columbus, Ohio, not later than December 15, 1948.

A Meteorologist Examination has been announced by the U. S. Civil Service Commission for filling positions in various Federal agencies in Washington, D. C., and vicinity. Positions in the United States Weather Bureau throughout the country and a few in the Territories and possessions of the United States and in foreign countries may also be filled. The salaries range from $3,727 to $6,235 a year.

A written test will not be required. To qualify, applicants must (a) have had 4 years of technical or scientific experience in meteorology or related fields; or (b) have completed a full curriculum of college study leading to a bachelor's degree which has included 20 hours of laboratory work in meteorology; or (c) have had any combination of such experience and education. In addition, all competitors must have had from 1 to 4 years of professional experience in meteorology.

Further information and application forms may be obtained from most first- and second-class post offices, from Civil Service regional offices, or from the U. S. Civil Service Commission in Washington, D. C. Applications will be accepted until June 30, 1949, in the Commission's Washington office.

Book Review

The Stars are Yours, by James S. Pickering. (The Macmillan Company, New York. $3.95.)

This book, as the above title indicates, is concerned with astronomy. It is not limited to a consideration of the stars only but deals also with the known facts related to the planets, comets, asteroids, and meteors. After a careful description of these objects which, astronomically speaking, are quite near to the earth, the author widens his outlook and explains the content of such terms as ecliptic, constellations, galaxy, nebulae, etc. Bound in the text is a 24-page form of photographs of astronomical subjects ranging from a total eclipse of the sun to some of the distant nebulae. These photographs are printed on enamel paper, to bring out the important details the better, and are accompanied by extensive legends which enable the reader to understand the photographs.

Another commendable feature of the book is a series of 24 constellation charts, one for each hour of right ascension, each one starting from the north star, or Polaris, which is centrally placed at the top. These charts are accompanied with text listing and describing the celestial objects to be found in them. These charts extend only to about thirty degrees south of the equator. Later in the book the author describes the southern constellations, basing the descriptions upon the chart of the southern sky, which is printed at the end of the book.

In an appendix of some 40 pages the author assembles various miscellaneous facts pertinent to the study of astronomy, so that the reader need not turn to other sources for answers to questions which may arise. All this makes it a valuable and desirable book to have at hand. It is written in a style that is comprehensible to any intelligent reader.

For the most part the author keeps the discussion within the bounds of well-established facts. In one chapter, which he entitles "Beyond the Horizon," he gives rein to the imagination and embarks upon a journey through the universe. Space travel may, indeed, sometime become a reality, although it is quite improbable that such a journey as is here pictured in the imagination will ever take place. Nevertheless, by this device the author succeeds in making more vivid the universe as it is thought to be today.

It is of especial interest to the reviewer to note that the book is dedicated to D. B. P., that is to David B. Pickering, the father of the author. It was he who in the late twenties and early thirties contributed a series of twelve most interesting papers to this journal under the title "The Astronomical Fraternity of the World." These papers were descriptive of the experiences which he and Mrs. Pickering had in visiting astronomical centers in widely separted parts of the world. He also gave of his time and his resources most generously in promoting the work of the American Association of Variable Star Observers. The charts he made are still in use. He did not live to see the book by his son, but, it is a reasonably safe surmise that he furnished much of the enthusiasm and inspiration for it, a fact which is graciously and appropriately attested in the dedication.

C.H.G.

Publications Received.—The publishers of POPULAR ASTRONOMY hereby acknowledge receipt of the following named publications and express their great appreciation of the courtesy shown on the part of those who have sent them.

Harvard Reprints:

No. 732. "A-Type Stars with Abnormal Specta," by Lawrence H. Aller.

No. 733. "The Spectrum of β Pegasi," by Dorothy N. Davis.

Contributions from the Mount Wilson Observatory:

No. 734. "Six-color Photometry of Stars. V. Infrared Radiation from the Region of the Galactic Center," by Joel Stebbins and A. E. Whitford.

No. 735. "The Postmaximum Spectrum of χ Cygni," by Paul W. Merrill.

No. 736. "The Solar Curve of Growth for Lines of *Ti*," by R. B. King and K. O. Wright.

No. 737. "The Absorption Lines within the Hydrogen Emission of Mira Ceti," by Alfred H. Joy.

No. 738. "High-Dispersion Spectra of ν Sagittarii," by Jesse L. Greenstein and Walter S. Adams.